

Moved to Opportunity:

The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children

Eric Chyn*

Department of Economics
University of Michigan

December 12, 2015

Job Market Paper

Preliminary and Incomplete. Please do not cite or distribute.

[Please click here for the most recent version of the paper.](#)

Abstract

This paper provides new evidence on the effects of moving out of disadvantaged neighborhoods on the long-run economic outcomes of children. My empirical strategy is based on public housing demolitions in Chicago which forced households to relocate to private market housing using vouchers. Specifically, I compare adult outcomes of children displaced by demolition to their peers who lived in nearby public housing that was not demolished. Displaced children are 9 percent more likely to be employed and earn 16 percent more as adults. These results contrast with the Moving to Opportunity (MTO) relocation study, which detected effects only for children who were young when their families moved. To explore this discrepancy, this paper also examines a housing voucher lottery program (similar to MTO) conducted in Chicago. I find no measurable impact on labor market outcomes for children in households that won vouchers. The contrast between the lottery and demolition estimates remains even after re-weighting the demolition sample to adjust for differences in observed characteristics. Overall, this evidence suggests lottery volunteers are negatively selected on the magnitude of their children's gains from relocation. This implies that moving from disadvantaged neighborhoods may have substantially larger impact on children than what is suggested by results from voucher experiments where parents elect to participate.

JEL Classification: I38, J18, J38.

Keywords: neighborhood effects, labor supply, public housing, vouchers.

*I would like to thank Brian Jacob for his help in accessing the data and his advice on this project. I am also grateful to Martha Bailey, John Bound, Charles Brown, John DiNardo, Susan Dynarski, Michael Eriksen, Jens Ludwig, Jeff Smith, Michael Mueller-Smith, Mel Stephens and Justin Wolfers for their feedback. This paper also benefited from my conversations with Jacob Bastian, Lasse Brune, Morgan Henderson, Sarah Johnston, Jason Kerwin, Johannes Norling, Bryan Stuart and Isaac Sorkin. I acknowledge fellowship support from a NICHD training grant to the Population Studies Center at the University of Michigan (T32 HD0077339). In addition, I am grateful for use of services and facilities at the Population Studies Center which is funded by a NICHD Center Grant (R24 HD041028). Please contact me at ericchyn@umich.edu.

1 Introduction

Over the past three decades, the number of U.S. children living in high-poverty neighborhoods has grown by nearly 80 percent to reach 20 million (Bishaw, 2014; Census, 1990). This increase has renewed concern over the long-run consequences of growing-up in disadvantaged areas. Theory suggests that a child’s odds of success are reduced if they live in impoverished neighborhoods where most adults are unemployed and peers engage in criminal activity (Wilson, 1987; Sampson and Groves, 1989; Massey and Denton, 1993).

Yet, quantifying the causal impact of neighborhoods on children has proven difficult largely because unobserved factors influence both selection into neighborhoods and individual outcomes. The best evidence to date comes from the Moving to Opportunity (MTO) experiment which randomly allocated housing vouchers to a sample of families living in low-income public housing. Recent analysis of MTO detected a positive impact on adult labor market outcomes for children who were young when their families moved and no detectable effect for older children (Chetty et al., 2015). While MTO provides important and credible estimates of neighborhood effects, some researchers have expressed concern that findings based on a group of motivated volunteers may not generalize.¹ For example, Durlauf (2004) writes: “[O]ne cannot extrapolate the [MTO] findings to the broader population of the poor.”

This paper provides new evidence on the impact of neighborhood conditions on a general population of disadvantaged children by exploiting public housing demolitions as a natural experiment. During the 1990s, the Chicago Housing Authority (CHA) began reducing its stock of public housing by selecting buildings with poor maintenance for demolition while leaving nearby buildings untouched. Residents of buildings selected for demolition received housing vouchers and were forced to relocate.

Public housing demolition in Chicago provides plausibly exogenous variation in the neighborhoods where displaced children grow up. My research design compares adult labor market outcomes of children from displaced households to their non-displaced peers that lived in the same public housing development. Because these two groups of children and their households were similar before

¹The MTO researchers have been careful in discussing the issue of external validity. Ludwig et al. (2001) write: “We hasten to note that because participation in the MTO program is voluntary, our estimates of the effects of relocation may be different from the effects of relocating a randomly selected group of families from poor areas.”

the demolition, any differences in later-life outcomes can be attributed to neighborhood relocation (Jacob, 2004).

Novel administrative data from Illinois makes it possible to match social assistance records to pre-demolition addresses of public housing to create a sample of displaced and non-displaced children and their households. In addition, the address information in assistance records allows me to verify that displaced households relocated to less disadvantaged neighborhoods. Three years after demolition, displaced households lived in neighborhoods with 25 percent lower poverty and 23 percent less violent crime relative to non-displaced households.²

Using employment data linked to the social assistance records, I find that displaced children grow up to have notably better labor market outcomes. Displaced children are 9 percent (4 percentage points) more likely to be employed as adults relative to their non-displaced peers. Further, displaced children have \$602 higher annual earnings – an increase of 16 percent relative to their non-displaced counterparts. The trajectory of these benefits is relatively stable across the range of adult ages that I observe for my sample of children.³

The estimates in this paper contrast with previous findings in the neighborhood effects literature.⁴ Unlike the recent extended analysis of MTO that revealed earnings benefits for the subgroup of children who were young (below age 13) when their families moved, this paper detects a positive impact regardless of the age when a child’s family moves.⁵ Moreover, these benefits accrue to displaced children whose family received standard vouchers, whereas the MTO study finds effects only for children receiving vouchers that were restricted for use in low poverty areas.

To understand these differences in results, I analyze a housing voucher lottery that occurred in Chicago shortly after the demolitions began. Similar to MTO, authorities in Chicago randomly allocated vouchers to households that were sufficiently motivated to sign up for this program.

²This change in neighborhood conditions is similar to the pattern for MTO volunteers that were randomly selected to be part of the unrestricted (“Section-8”) housing voucher treatment group.

³In the last year of my administrative data, individuals in my sample were between ages 21 and 32 and experienced demolition when they were 7 to 18 years old. The results are not sensitive to including even younger children.

⁴This paper also relates to previous work on the impact of Chicago’s public housing demolitions. I extend Jacob (2004) which studies the short-run impact of public housing demolition on schooling outcomes.

⁵Note that Sanbonmatsu et al. (2011) also analyzed children who participated in MTO finding no detectable impact on labor market outcomes. The more recent MTO analysis by Chetty et al. (2015) differs from this previous work by focusing on younger (below age 13) children who were not old enough to have completed their education at the time that data was collected for Sanbonmatsu et al. (2011). In other major work on neighborhood effects, Oreopoulos (2003) found no improvement in labor market outcomes for adults that grew-up in very different public housing projects in Toronto.

While families that won the lottery moved to lower poverty neighborhoods, the effects of winning a voucher on labor market outcomes of children are small and not statistically significant.⁶ The contrast between the effects in the lottery and demolition samples remains even after re-weighting the demolition sample to adjust for the fact that lottery households are relatively more advantaged.

Comparing the lottery and demolition results suggests that children who move through voluntary mobility programs have lower gains to relocation. This “reverse Roy” pattern of negative selection parallels education studies which find that children who would benefit most from attending charters or other high-quality schools are the least likely to apply (Walters, 2014; Hastings et al., 2008). In addition, these results are consistent with recent analysis that suggests children with the largest achievement gains from Head Start are the least likely to enroll (Kline and Walters, 2014).

I conclude that there are significant benefits to relocating children (of any age) from public housing. Back-of-the-envelope calculations suggest that a child who moves out of public housing due to demolition has about \$45,000 higher total lifetime earnings (\$12,000 in present value). The increased tax revenue associated with this earnings gain exceeds the (average) cost of relocating public housing residents. This suggests that efforts to improve long-run outcomes of disadvantaged children will yield net gains for government budgets.

2 History of Public Housing Demolition in Chicago

During the 1990s, the Chicago Housing Authority (CHA) had the third largest public housing inventory in the U.S., providing services to nearly five percent of the city’s population (Popkin et al., 2000; Jacob, 2004). Seventeen housing developments (also known as “projects”) spanned the city providing homes specifically for families with children. Each project consisted of a collection of apartment buildings built in close proximity. Many of these buildings were large high-rise structures providing 75 to 150 units of housing.

Low-income households were eligible to live in public housing if their income was at or below 50 percent of Chicago’s median income. Because public housing is not an entitlement, eligible families typically spent years on waiting lists and usually accepted the first public housing unit that was

⁶To compare the effects on adult labor market participation in the lottery and demolition samples, I test that the difference in point estimates is zero, and the resulting p -value is 0.13. Results for labor market earnings are much less precise in the lottery sample, and a test of the difference in point estimates across samples has a p -value of 0.43.

offered to them.⁷ The vast majority of public housing residents during this period in Chicago were African-American, and a large share were single-parent, female-headed households.

The demolition of public housing in Chicago during the 1990s began as a reaction to serious housing management problems.⁸ By the end of the 1980s, chronic infrastructure problems plagued much of the public housing stock which had been built poorly during the 1950s and 1960s. Few city officials believed that the CHA could effectively address these maintenance issues after a series of scandals revealed that housing authorities had mismanaged public funds. With this in mind, authorities laid plans to replace project-based housing assistance with vouchers and gradually eliminate public housing through building demolition.⁹ Although the city wanted to eventually eliminate much of the housing stock, funding limitations implied that only a small number of demolitions occurred in the 1990s.

The first demolitions in Chicago stemmed from a variety of events and circumstances that were sometimes unforeseen (Jacob, 2004).¹⁰ For example, in January 1999 pipes burst in several high-rise buildings in the Robert Taylor projects causing flooding that shut down heating systems. Residents were forced to evacuate four buildings, and the CHA subsequently closed these buildings for demolition (Garza, 1999a). Similarly, harsh winter weather damaged several buildings in the Henry Horner Homes project, prompting the CHA to close these buildings for demolition (Garza, 1999b). When not reacting to specific crises, the CHA generally sought to close buildings that had the most severe maintenance issues.¹¹

When the CHA selected a building for demolition, the authority provided Section 8 housing

⁷For example, over 30,000 households were on the CHA public housing waiting list during the mid-1990s (Jacob, 2004). Households must wait as their request for housing assistance rises to the top of the queue. Once at the top of the list, a household that is unsatisfied with their offer can reject their assignment, but they must then return to the bottom of the wait list. Further, due to the same high demand for services, there is also little opportunity to transfer between housing units after entering the public housing system.

⁸For a detailed history of public housing in Chicago, see Popkin et al. (2000).

⁹A number of federal housing policy reforms also facilitated public housing demolition in Chicago. Specifically, the creation of the HOPE VI program in 1993 helped provide funding for demolition. The CHA was one of the largest recipients of HOPE VI funding, receiving nearly \$160 million during the 1990s.

¹⁰Note that the analysis in this paper only pertains to building demolitions that occurred from 1995-1998 which preceded public housing demolitions under the Chicago Housing Authority's "Plan for Transformation". This focus on demolitions during the 1990s allows me to study long-run outcomes and also corresponds to the period studied in Jacob (2004).

¹¹An additional motivation for building closure was related to criminal activity. For example, snipers located on the roof of a Cabrini Green building shot 7-year-old Dantrell Davis in 1992. The building from which the shots were fired was permanently closed after the shooting and later demolished in 1996 (Hawes, 1992). I exclude the projects and the buildings where these crime-based closures occurred from the analysis in this paper.

vouchers to displaced residents which allowed recipients to rent housing on the private market.¹² Alternatively, residents of affected buildings were also given the option of applying to transfer to another unit in their current project or transfer to another unit in a different CHA project. In the case that a resident selected the voucher offer, the CHA paid for all moving expenses. Note that the CHA provided few support services such as housing counseling during the period of my study.¹³

Typically, the voucher subsidy was equal to the difference between the gross rent or the local Fair Market Rent (FMR) and the family's required rent contribution (30 percent of adjusted income). The FMR was equal to the 40th percentile of the local private-market rent distribution. Families that obtained a housing voucher were able to keep the voucher as long as they remained eligible for assistance. Finally, the transition to vouchers from public housing should *not* mechanically affect the budget set of assisted households because the program and rent rules for vouchers and project-based assistance were similar.¹⁴

3 Expected Effects of Demolition on Children

The expected effect of public housing demolition is related to the relocation decision of affected households. One possibility is that displaced households used their vouchers to move to lower poverty neighborhoods. In this case, theory suggests that children may benefit because relatively affluent adults serve as role models that shape norms and social identity (Wilson, 1987; Akerlof and Kranton, 2000). Living in a less disadvantaged neighborhood can also affect a child's long-run outcomes by providing exposure to higher-income peers that can provide job information and referrals.¹⁵ Relatedly, lower poverty areas may provide displaced parents with better access to job-finding networks, which implies that they may be more likely to work and invest in goods that promote child development.

While these mechanisms suggest children should benefit from moving to low-poverty neighbor-

¹²Prior to the beginning of Chicago's public housing demolitions, low-income households had no ability to access housing vouchers because the CHA stopped allowing requests in 1985 (Jacob and Ludwig, 2012).

¹³Later initiatives by the CHA included providing relocation support to residents who were displaced by building demolitions that occurred during the 2000s (Popkin, 2012).

¹⁴For the interested reader, a full list of program rules for housing vouchers is contained in Appendix Section A3.

¹⁵In addition, living in a lower poverty areas may reduce a child's exposure to peers who commit crime. This is particularly relevant in the context of Chicago where many poor neighborhoods also have very high rates of crime. Damm and Dustmann (2014) provide evidence that a child's long-run outcomes are causally affected by the share of youth criminals living in their neighborhood.

hoods, existing empirical studies provide mixed support for this prediction. For example, analysis of Chicago’s Gautreaux program found that low-income children who moved to the suburbs had much better outcomes than their peers whose families moved within the city (Rosenbaum, 1995).¹⁶ However, Oreopoulos (2003) failed to detect any impact of living in a lower poverty area on children in Toronto. In addition, Chetty et al. (2015) find no evidence of benefits for children who were older when their family moved to a low-poverty area through MTO. In relation to the present paper, the MTO results are the most relevant comparison given its similar focus on high-rise public housing in major U.S. cities. However, the results from MTO may differ from the demolition context because MTO families volunteered to participate.

Another way in which demolition and relocation may affect children is through changes in the quality of schooling. A variety of studies provide credible evidence that attending schools with better teachers and smaller classes generates notable gains, especially in terms of long-run labor market outcomes (Chetty et al., 2011; Fredriksson et al., 2013; Fryer and Katz, 2013; Chetty et al., 2014). Yet, previous studies of housing assistance programs suggest that families typically *do not* use vouchers to relocate to areas that give their children access to better schools. In the MTO evaluation, there was little impact on child school quality although households moved to areas with notably lower poverty (Sanbonmatsu et al., 2006). Moreover, the most relevant evidence for the demolition context comes from Jacob (2004), who studied the short-run impact of public housing demolition on education outcomes. He finds that there is little difference in school quality for displaced and non-displaced children after relocating due to demolition.¹⁷

Finally, displaced children may have different outcomes even if their households did not relocate to less disadvantaged neighborhoods. One idea proposed by sociologists posits that the physical design and density of public housing projects fosters criminal and other negative behavior (Newman, 1973). Hence, relocation to less-dense private market housing can impact child development

¹⁶The Gautreaux program provided housing vouchers to a limited set of low-income families in Chicago in the late 1970s. As part of this voucher program, counselors recommended apartments in lower-poverty areas of Chicago or the surrounding suburbs. Researchers studying the Gautreaux program argue that this process resulted in a quasi-random assignment of households to new neighborhoods. In support of this argument, Popkin et al. (1993) report that 95 percent of program participants accepted the first apartment offered. However, Shroder and Orr (2012) argue that it is unclear whether the Gautreaux setting approximated random assignment because there are statistically significant differences in observed characteristics between the set of participants that moved to the suburbs or within Chicago.

¹⁷As will be discussed in Section 7, Jacob (2004) also finds few detectable effects on student outcomes such as test scores and grades. This suggests that an impact on long-run child outcomes is not due to changes in schooling achievement.

irrespective of changes in exposure to neighborhood poverty.

4 Data Sources and Sample Construction

The data that I use to test whether demolition has an impact on long-run outcomes of children is drawn from multiple administrative sources. Specifically, I combine building records from the Chicago Housing Authority (CHA) and social assistance (i.e., TANF/AFDC, Food Stamp and Medicaid) case files (1989-2009) from the Illinois Department of Human Services to create a sample of children that lived in public housing and were affected by demolition during the 1990s. I obtain additional information on long-run outcomes by merging this sample of children with unemployment insurance wage records (1995-2009) from the Illinois Department of Employment Security. In addition, I also obtain measures of baseline (prior to demolition) characteristics by linking the sample to arrest records from the Illinois State Police.¹⁸ The employment and police data are linked to the social assistance case records using identifiers created by Chapin Hall at the University of Chicago, a research institute and leader in administrative-data linking. In the next sections, I describe my sample and the data construction in more detail.

4.1 Sample of Public Housing Buildings

My analysis focuses on a subset of public housing projects and buildings listed in CHA building inventory records from the 1990s. Specifically, I examine non-senior-citizen projects that experienced building closure and demolitions from 1995-1998 which were part of the initial wave of housing demolitions associated with HOPE VI grants.¹⁹ I also restrict attention to high-rise buildings (defined as having 75 units or more).²⁰ Finally, I exclude projects where documented evidence suggests that building demolition was correlated with unobserved tenant characteristics. Specifically, I exclude the Cabrini Green and Henry Horner projects.²¹

¹⁸I also use the police data for supplementary analysis of adolescent criminal behavior.

¹⁹Note that I exclude high-rise projects that did not have any buildings closed due to demolition. This is because my empirical specification includes project fixed effects to account for systematic differences *across* projects. Hence, projects that did not experience demolition would not contribute to the identification of the impact of demolition.

²⁰In general, low- and mid-rise buildings did not experience the same type of sudden and abrupt demolition process as observed in high-rises.

²¹As highlighted by [Jacob \(2004\)](#), the housing authority chose to demolish buildings at the Cabrini-Green Homes that were associated with gang activity and crime. I also exclude the Henry Horner project because selection of buildings for demolition and voucher distribution was notably different at this site ([Vale and Graves, 2010](#)). This non-standard process was due to an earlier lawsuit initiated by the Henry Horner residents.

The final sample contains 53 high-rise buildings located in seven projects. I obtain the date when a building was closed from Jacob (2004) which determined the year of closure by examining CHA administrative data on building occupancy supplemented by qualitative sources.²² Based on this information, there were 20 demolished (treated) and 33 stable (control) buildings during my study period. Note that stable buildings are defined as those that did not close during the 1995-2002 period.

4.2 Linking Households to the Public Housing System

To create my main analysis sample, I exploit the fact that social assistance records provide the exact street address for welfare recipients. Specifically, I link welfare recipients who have a street address that matches a building in my public housing project sample in the year prior to notification of demolition. Note that by focusing on addresses in the assistance data in the year *before* demolition, the sample definition is unrelated to any impact that demolition has on public assistance participation. Overall, the assistance data contains 5,676 adult recipients that live in public housing in the year before demolition. Since the sample of public housing buildings contains 7,770 individual apartments, this suggests that my assistance sample covers at least 73 percent of the households living in the demolition sample of buildings (assuming there are no vacant apartments). The high coverage rate is not surprising given that the disadvantaged status of the Chicago public housing population implies that many residents will receive some form of public assistance.²³

Finally, I focus on children aged 7 to 18 in the year that building demolition takes place at their project. With this sample, I observe adult (age > 18) outcomes for at least three years and at most 14 years for each child. Moreover, this age restriction allows me to compare my results directly to the analysis of children in the final impact evaluation from MTO.²⁴ The final data contains 5,250 children from 2,767 households. Using this set of children from project-based public housing, I create a panel at the person-year level which covers the period from demolition to 2009, the last year of my administrative data on labor market and welfare outcomes. The number of observations per individual is determined by the demolition date. For example, residents of projects

²²Appendix Section A4 provides further details on the process for determining the year of demolition.

²³Only 15 percent of households living in Chicago public housing had an employed member, and the average CHA household income was \$6,936 per year (Popkin et al., 2000).

²⁴In Appendix Table A8, I show that my main results are not sensitive to changing the sample definition to include even younger children who will be just entering the labor market in the final years covered by my employment data.

that had demolitions in 1995 will have 14 observations in their panel. I merge this panel with the administrative data on labor market outcomes, social assistance receipt and criminal arrests.

5 Empirical Approach

5.1 Estimating the Reduced-Form Effect of Demolition

As in [Jacob \(2004\)](#), I study the impact of demolition by exploiting the fact that the CHA selected a limited number of buildings for demolition within each public housing project during the 1990s. Hence, my empirical strategy compares children who lived in buildings selected for demolition to their counterparts living in non-demolished buildings.²⁵ The former is the treatment group which is displaced from public housing. To the extent that displaced and non-displaced individuals are randomly assigned within the same project, subsequent differences in outcomes can be attributed to the demolition and relocation.

I use the following linear model to study the impact of demolition on outcome y for children,

$$Y_{it} = \alpha + \beta D_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \quad (1)$$

where i is an individual and t represents years. The indexes $b(i)$ and $p(i)$ are the building and project for individual i . The terms δ_t and $\psi_{p(i)}$ are year and project fixed effects, respectively. The vector X_i is a set of control variables (e.g., age) included to improve precision. The dummy variable $D_{b(i)}$ takes a value of one if an individual lived in a building slated for demolition. Hence, β represents the net impact of demolition on children's outcomes.

In addition, I also estimate two augmented versions of Equation 1. First, I estimate a model which includes interactions for gender.²⁶ This analysis is motivated by a large body of previous empirical work that documents significant heterogeneity by gender. For example, many of the benefits detected in the MTO evaluations were found for girls but not for boys ([Kling et al., 2005](#)).

²⁵For example, in the Robert Taylor Homes project, building #1 was slated for demolition in 1995 while other high-rises in Robert Taylor were left untouched at that time. Residents of this latter group of stable buildings can be used as a comparison group that holds constant characteristics specific to residents at Robert Taylor Homes.

²⁶Formally, the augmented model I estimate is:

$$Y_{it} = \beta_g G_i D_{b(i)} + \beta_b (1 - G_i) D_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}$$

where G is a dummy variable that takes the value of one if individual i is female. In this equation, β_b is the average difference in outcomes between males in the treatment and control group while β_g is the average difference for females.

In addition, [Anderson \(2008\)](#) analyzes data from several education interventions finding that all benefits accrued to girls, with no statistically significant long-term benefits for boys. Second, I also explore how treatment effects vary with age by estimating a model which interacts an individual’s age in a given year and the treatment indicator. Hence, this specification compares displaced (treated) and their non-displaced peers at each age of measurement observed in my sample.

Estimates of β in Equation 1 have a causal interpretation if the CHA’s selection of buildings for demolition was unrelated to resident characteristics. The historical evidence suggests that this condition is plausible because building maintenance issues were the main concern when selecting buildings for demolition. Moreover, there should be little difference between residents living in demolished and non-demolished buildings because the tenant allocation process restricts the ability of households to sort into different buildings. Recall that most families spend years on the public housing waiting list and accept the first unit that becomes available. Finally, in the next section, I provide empirical support for this assumption that building demolition is unrelated to resident characteristics by showing that my sample of displaced and non-displaced residents have similar observed characteristics in the year prior to demolition.

5.2 Comparing Treated and Control Individuals in the Year Prior to Demolition

The validity of my research design depends on whether the selection of buildings for demolition was uncorrelated with characteristics of children living in public housing. To provide support for this assumption, I exploit the comprehensive nature of my administrative data to compare children living in buildings marked for demolition (treated) and stable (control) buildings. Specifically, I examine crime and demographic characteristics measured in the (baseline) year prior to demolition.

Table 1 compares children living in treated and control buildings by estimating a regression model where the dependent variable is a child characteristic measured in the year before demolition and the key independent variable is an indicator for living in a treated building. Column (1) of the table shows means for various outcomes for all non-displaced children living in stable public housing buildings. The second column reports the mean difference between control and treated individuals from the regression model. If selection of buildings was uncorrelated with child characteristics, we expect that the mean difference would equal zero. The table shows that the mean difference is never statistically different from zero for all measures of past criminal activity and demographics

in my sample.²⁷ In addition, Table 1 shows there is no difference in schooling outcomes at baseline by reproducing the balance test results from [Jacob \(2004\)](#), who studied the impact of demolition on schooling outcomes.²⁸ Columns (3) through (6) similarly show no detectable difference between displaced and non-displaced youth by gender subgroup.²⁹

I also examine whether there are detectable differences in the baseline characteristics of adult (age > 18) residents of public housing. One important limitation in this analysis is that the data only includes adults living in public housing who are observed in the social assistance case files. Table 1 examines this sample of adults in Columns (7) and (8) in terms of demographic, criminal and labor market characteristics. Similar to the exercise for youth, Column (7) reports the mean for non-displaced adults measured in the year before demolition. Column (8) reports the difference between displaced and non-displaced adults as calculated from a regression model. Reassuringly, adults living in buildings marked for demolition do not appear statistically different in terms of past criminal activity or labor market activity. Adults in treated buildings are one year older, but the magnitude of this difference is small relative to the mean adult age.

5.3 Testing for Attrition and Spatial Spillovers

Administrative data allows me to avoid many concerns over sample attrition and missing data that are problematic in other studies. If an individual works in the state of Illinois in any quarter from 1995 to 2009, I observe earnings as reported to the Illinois unemployment insurance (UI) program. At the same time, one may be concerned that my estimates are biased if children displaced by demolition are more likely to move out of state. In this case, my administrative data would suffer from a missing data problem: an individual who moves out of state will have zero earnings in the Illinois data even if they are working in their new state of residence.

To address concerns over attrition, I follow [Grogger \(2013\)](#) and use terminal runs of zeros to measure permanent out-of-jurisdiction attrition. Intuitively, the idea is that attrition has a distinc-

²⁷Note that juvenile arrests come from the Illinois State Police (ISP) data. Prior to 1998, the arrest data for juveniles in the ISP data is limited to serious felonies. After this date, revisions in the Illinois Juvenile Court Act allowed for the submission of juvenile misdemeanor arrests into the ISP database, resulting in more complete coverage of juvenile criminal activity.

²⁸The sample of treated and control public housing buildings used in this paper matches the sample used in [Jacob \(2004\)](#) except I exclude buildings from the Henry Horner project where there was a distinct process for public housing demolition. All of my results are robust to including Henry Horner in my analysis.

²⁹For schooling outcomes, there are no gender specific balance tests because [Jacob \(2004\)](#) did not provide balance tests for schooling outcomes separately by gender.

tive pattern: when an individual moves out of state, all of their subsequent entries in administrative panel data from their original location are zeros. As detailed in Appendix Section A1, I construct this measure of attrition based on terminal zeros and test for imbalance across treatment and control groups. This analysis reveals no evidence that children displaced by demolition are any more likely to attrit from the administrative data than non-displaced children.

A final concern for my empirical results are spatial spillovers stemming from demolition. In other words, the control group of non-displaced children could be affected by the demolition of neighboring buildings and the relocation of their peers. This would bias my estimates upward if non-displaced children are worse off due to demolition. To test for the existence of spillovers, I augment Equation 1 with additional indicators for living in a stable building that is adjacent to a demolition building,

$$Y_{it} = \alpha + \beta' D_{b(i)} + \pi N_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}, \quad (2)$$

where $N_{b(i)}$ is an indicator that a public housing building borders (is adjacent to) a demolition-targeted building. The omitted group in the regression is the set of children living in stable buildings located farther away from a demolished building. This specification tests for spillovers on stable buildings under the assumption that social interactions between buildings within a project decrease with distance. Appendix Table A2 presents the results of this specification for labor market and welfare outcomes. I find no evidence of this type of spillover given that the analysis fails to reject the null $\pi = 0$ across outcomes.

I also test for the existence of general spillovers at the project level. For instance, a concern is that demolition reduced social cohesion in all remaining (control) buildings at the project and this could affect long-run child outcomes.³⁰ To address this, I conduct an event-study of Census-tract-level homicides as a proxy for social conditions at and around public housing projects.³¹ Formally,

³⁰Previous research on the demolitions in Chicago finds that there is no evidence of this type of negative spillover affecting non-displaced residents. [Aliprantis and Hartley \(2015\)](#) estimate that there is a *decrease* in violent crime after a project experiences building demolition. Yet, in contrast to the present study, they study the impact of demolition on neighborhood-level crime over a longer time period and include demolitions that occurred after 1998.

³¹I focus on homicides because tract-level data on property or other forms of criminal activity are unavailable for Chicago for my period of study from 1995-1998.

I use the following specification: ³²

$$H_{it} = \mu_i + \sum_{j=-4}^{-1} \pi_j \mathbf{1}(t - t^* = j) + \sum_{j=1}^{10} \tau_j \mathbf{1}(t - t^* = j) + \delta_t + \epsilon_{st} \quad (3)$$

where the dependent variable H_{it} is homicide rate for Census tract i at year t . The terms μ_i and δ_t are tract and year fixed effects, respectively. In the notation, t^* is the year t in which a particular tract experiences a public housing demolition directly (or there is a demolition within one mile of the tract). The dummy variables $\mathbf{1}(t - t^* = j)$ indicates that an observation in year t is j -periods before or after demolition occurs. For example, the dummy variable $\mathbf{1}(-1 = j)$ indicates that the observation y is one year before the policy is implemented.

I restrict the estimation sample to include tracts that (1) that contained public housing with at least one building demolition or (2) are within one mile of a public housing demolition site. The data contain observations that are at most four years before demolition and up to 10 years after a demolition. I choose four pre-periods because the bulk of the demolitions I consider occur in 1995 and my homicide data start in 1991. Note that year effects δ_t are identified using data from locations that have not yet or already have had a demolition. The Figure A1 plots the coefficients π_j and τ_j along with the 95-percent confidence interval. The results show that there is no detectable impact of demolition on the tract homicide rates following public housing demolition.

6 Main Results

Since the expected effects of demolition depend on relocation choices, I begin by showing that displaced households with children moved to less disadvantaged neighborhoods compared to their non-displaced peers. Next, I study labor market outcomes, finding that there is a large positive impact on long-run employment and annual earnings. Yet, there is no detectable impact of demolition on any measure of public assistance utilization.

6.1 The Impact of Demolition on Household Location

Housing vouchers increase housing location choice (relative to project-based housing assistance) and can potentially reduce low-income household exposure to poverty and other forms of neigh-

³²See [Jacobson et al. \(1993\)](#) for further discussion of event-study specifications.

neighborhood disadvantage. Increased choice may be particularly important for families on housing assistance since public housing projects tend to be located in the most distressed areas of cities. For example, the public housing buildings in my Chicago sample reside in Census tracts where the poverty rate – defined as the fraction of persons below the federal poverty line – was about 78 percent. To put this figure in perspective, Census tracts with 40 percent or more households falling below the poverty line are typically classified as extreme poverty tracts (Coulton et al., 1996). According to 2000 Census data, only 12.4 percent of the U.S. population had income below the poverty line (Bishaw, 2014).

I test whether displaced public housing residents move to better quality neighborhoods using address information from social assistance case records. The primary concern with this analysis of post-demolition location is that address data is only available if some member of a household received assistance such as AFDC/TANF, Foodstamps or Medicaid. Hence, this analysis may be biased if demolition has an impact on participation in social assistance programs.³³ In subsequent Section 6.3, I find that there is no detectable difference in the probability that displaced children are on social assistance in the years following demolition.

Table 2 shows that displaced (treated) households are less likely to live in public housing and moved to better quality neighborhoods relative to their non-displaced (control) peers. Column (2) shows that three years after demolition treated households with children are about 80 percent less likely to reside in public housing and live in Census tracts that average a 20 percent lower poverty rate relative to control households. In addition, their neighborhoods have less crime: treated households lived in neighborhoods with about 23 (about 33 percent) fewer violent crimes per 10,000 residents. Overall, these statistics on neighborhood relocation are similar to those reported in Jacob (2004), who relied on a different administrative source (specifically, he uses data from the Chicago Public Schools) to examine changes in children’s location.³⁴

Figure 1 presents densities of neighborhood (tract) poverty rates to help characterize the cumu-

³³If demolition reduces the likelihood that a household uses social assistance, they will not have an active record in the social assistance data which implies that I will not observe their address history. The direction of this bias for the mobility analysis then depends on what kind of neighborhood these households selected.

³⁴Moreover, the similarity between these results and Jacob (2004) should provide further reassurance against concern over the impact of demolition on the address histories that I construct from social assistance records. This is because Jacob (2004) analyzed relocation outcomes using Chicago Public School data where there is no concern over differential attrition due to the impact of demolition on use of social assistance.

lative impact of demolition on neighborhood quality.³⁵ Specifically, the poverty rates in the figure are duration-weighted averages over all the locations at which a household lived at since demolition. Separate densities are presented for displaced (solid) and non-displaced (dashed) households. This figure indicates that a large share of displaced residents relocated and lived in neighborhoods with notably lower poverty rates relative to residents of stable buildings. Nearly 44 percent of treated households lived in neighborhoods with poverty rates less than 40 percent (the threshold for classification as an extreme poverty neighborhood). Overall, the change in neighborhood condition in my sample is similar to the pattern for MTO volunteers that were randomly selected to be part of the unrestricted (“Section-8”) treatment group (Kling et al., 2005; Sanbonmatsu et al., 2011).

The difference in neighborhood quality also shrinks over time. Column (4) of Table 2 shows that there is no detectable difference in neighborhood quality eight years after the demolition. This occurs because the vast majority of the control group moves out of public housing by this time.³⁶ To further explore the impact on neighborhood quality over time, Figure 2 plots the difference in the (tract) neighborhood poverty rate in each post demolition year. Unsurprisingly, the difference in neighborhood poverty is largest in the first year after demolition: displaced households live in neighborhoods with about 28 percentage points lower poverty relative to non-displaced households. After this point, the difference in poverty rate begins to attenuate to nearly 13 percentage points by year three. Eventually there is no detectable difference in neighborhood poverty eight years after demolition when the youngest children in my sample (age 7 at baseline) may be leaving their parents’ household.

6.2 Adult Labor Market Outcomes of Children

Table 3 examines the impact of demolition on children’s adult labor market outcomes by presenting results from Equation 1. The point estimate reported in Column (2) show that children (age 7 to 18 at baseline) whose households were displaced have higher labor-force participation and earnings during their adult working years (age > 18). On average, children who were displaced

³⁵This analysis uses the tract poverty rate associated with a household’s address in a given year which is obtained from social assistance case records. When a household does not have an active case in a given year, no address is observed. For the duration weighting, I only consider years in which an address (and poverty rate) is observed for a given household.

³⁶Recall that the set of control (non-demolished) buildings are defined as those that did not close during the 1995-2002 period. This implies that much of the movement by control households out of public housing (even in the long-run) was not due to later waves of demolition.

are 4 percentage points (9 percent) more likely to be employed and have about \$600 higher annual earnings.³⁷ While I do not directly measure hours worked, Table 3 also shows that the probability of earning more than \$14,000 – the equivalent of working full time (35 hours a week) at \$8 per hour for 50 weeks – increases by 1.3 percentage points (13 percent). Overall, these results show that demolition and subsequent relocation is strongly associated with better adult labor-market outcomes for children.

Table 4 presents estimates from a modified version of Equation 1 which allows treatment effects to vary by gender. The point estimates show that the positive impact detected for the full sample is driven mainly by girls. Relative to non-displaced peers, girls are 6.6 percentage points (13 percent) more likely to be employed and have \$806 (18 percent) higher annual earnings. The corresponding effects for boys are less precisely estimated although the point estimates for all outcomes are positive.³⁸

I also explore how labor market activity evolves as my sample ages by interacting an individual’s age in a given year (hereafter “age of measurement”) and the treatment indicator.³⁹ Figure 3 plots each point estimate for the estimated labor market treatment effect at each age for adult employment and earnings. Note that age of measurement on the x -axis ranges from 19 to 32 because the oldest child in my sample is age 32 in the last year of my sample. Despite larger standard errors at older ages, Panel (a) shows that the positive treatment effect for employment is relatively stable as the sample ages. Panel (b) shows that the earnings treatment effect is relatively stable although there is a slight positive trend and the point estimates are approximately \$5,000 after age 28.

Figure 4 examines the evolution of treatment effects separately for very young (age < 13 at baseline) and older children (age 13 to 18 at baseline). Note that I restrict the age of measurement because younger children are age 26 in the final year available in my administrative data. Overall,

³⁷Recall that these results are from analysis of a panel of employment and earnings based on IDES data. If an individual is not present in the IDES data, their earnings in that year are zero. All monetary values are in 2012 dollars.

³⁸Note that I cannot reject the equality of treatment effects for male and female earnings (p -value = 0.18). However, I can reject the hypothesis of the equality of treatment effect estimates for male and female labor participation (p -value = 0.07).

³⁹Note that the treatment effect for older ages (e.g. age 32) are only identified using children who were relatively old at the time that displacement occurred. Again, this is because children who were displaced when they were younger have just entered adulthood by the end of my sample. For example, the youngest child in my sample is age 7 at the time of demolition and age 21 in 2009, which is the last year in my administrative data on employment outcomes.

this analysis reveals two important findings. First, the treatment effects for older children (in red) are always positive (although not precisely measured) and shows little trend over time. Second, the analysis of younger children reveals that there is a slight increase in the size of the treatment effect point estimates at older ages. This pattern is consistent with a recent long-run evaluation of MTO evaluation by [Chetty et al. \(2015\)](#) which reveals that treatment effects for MTO children younger than age 13 are positive starting in their mid-twenties. However, [Chetty et al. \(2015\)](#) tend to find negative treatment effect point estimates for MTO children that are older at baseline (age 13 to 18), whereas I find that the point estimates for the effects on older children are always positive.⁴⁰

I also examine additional types of heterogeneity in the labor market response to demolition. Panels (a) and (b) in Appendix Table A4 present results for labor market participation and earnings, respectively. Each row of the table reports descriptive statistics and results specific to a given subgroup based on baseline characteristics. The results for younger (age < 13) and older (age 13 to 18) children do not show any heterogeneity based on age; however, this average difference masks the notable heterogeneity of treatment effects by age of measurement shown in Figure 4. The additional subgroup results paint a mixed picture of how treatment effects are related to baseline measures of household disadvantage. On the one hand, children from (more disadvantaged) households with no working adults have large treatment effects, and there are no detectable effects for children from households that have at least one working adult. On the other hand, children from (more disadvantaged) households with adults who have at least one prior arrest have no detectable treatment effects, while there are large treatment effects for children from households in which no adult has a past criminal arrest.⁴¹

Finally, Figure 5 and Appendix Table A3 further explore heterogeneity in the impact of demolition by estimating quantile treatment effects. Note that the lower bound of the x -axis on the figure is restricted to the 60th percentile because a large fraction of earnings are equal to zero.⁴² This analysis examines the treatment effect for particular percentiles of the earnings distribution and *does not* measure the effect for any particular individual in the sample. The pattern of the point estimates show a notable degree of heterogeneity in the earnings response with the treatment effects

⁴⁰For more on this comparison with recent re-analysis of MTO, see Figure 1 and Table 3 of [Chetty et al. \(2015\)](#).

⁴¹At the same time, it is worth noting that I cannot reject the equality of treatment effects across these different subgroups.

⁴²Specifically, the descriptive statistics in Panel A of Appendix Table A3 show that 48 and 67 percent of annual earnings for males and female, respectively, are zero.

generally increasing for higher quantiles. At the 85th percentile, the quantile treatment effect is about \$2,100, which is a 22 percentage point increase relative to the same percentile in the control distribution.

6.3 Adult Welfare Receipt of Children

Demolition and neighborhood relocation may also affect welfare receipt through many of the same mechanisms that link neighborhood conditions and labor-market outcomes. For example, [Bertrand et al. \(2000\)](#) examine U.S. Census data and find that use of social services and public assistance is affected by the usage rate of neighbors in one’s social network (measured by language spoken). In this section, I test whether there is any impact of displacement due to public housing demolition on public assistance receipt (TANF/AFDC, foodstamps or Medicaid).

Table 5 presents results from Equation 1 in column (2), which shows no detectable impact of demolition and relocation on utilization of AFDC/TANF, foodstamps or Medicaid services across years. The point estimates are generally very small (less than or equal to 0.01), and the 95-percent confidence intervals generally rule out effects larger than negative or positive 3 percentage points. Columns (4) and (6) present results by gender and show little detectable heterogeneity.

This lack of effects may seem initially surprising given that the positive treatment effect on labor market activity should reduce reliance on social assistance. However, the intensity of disadvantage in this sample of children means that even sizable gains in labor-market activity are not sufficient to reduce eligibility for social assistance. For example, the mean annual earnings for non-displaced (control) in my sample is about \$3,700, and the reduced form impact of demolition is about \$600 which implies that the average displaced (treated) households will still be below the maximum annual income limits for foodstamps (\$25,000), TANF (\$7,000) and Medicaid (\$26,000).⁴³ Even at the 90th percentile of the earnings distribution, treated individuals still qualify for both foodstamps and Medicaid assistance.⁴⁴ Hence, the detected treatment effects of labor market participation and earnings would not reduce social program eligibility for the vast majority of individuals in my sample.

⁴³Social service eligibility depends on both income and family size. The maximum income allowance described in the text applies to a family size of three. Details on Illinois TANF, foodstamp and Medicaid eligibility come from [DHS \(2015\)](#), [DHS \(2014\)](#) and [Brooks et al. \(2015\)](#), respectively.

⁴⁴For descriptive statistics on the quantiles of the earnings distribution, see Appendix Table A3.

7 Mediating Mechanisms

Why does demolition have such a large impact on the adult labor market outcomes of children? In addition to the mechanisms described in Section 3, displaced parents may be more likely to work and use the additional household income to invest in child development (Black et al., 2014). To test for this parental channel, Table 6 explores whether there is any impact of relocation due to demolition on labor market outcomes of parents.⁴⁵ Column (2) shows that the point estimates are consistently small and the effects are never statistically different from zero. For example, the reduced form effect on labor market participation represents less than a one percent effect ($=0.004/0.489$).⁴⁶ Overall, these results are consistent with previous analyses of MTO which found no detectable impact of vouchers for adults (Sanbonmatsu et al., 2011).

Another possibility is that living in a less disadvantaged neighborhood with less crime could possibly affect teenage criminal behavior. Indeed, Damm and Dustmann (2014) show that children who live in areas with a higher share of youth criminals are more likely to commit crime when they grow older. In this way, moving to lower poverty neighborhoods may boost labor market outcomes by affecting the likelihood of committing crime and being arrested. I test this hypothesis by examining the impact of demolition on teenage arrests using data from the Illinois State Police. Table 7 presents results that suggest that the positive impact of relocation due to demolition does not arise from reductions in adolescent criminal behavior. Column (2) reports results from estimating Equation 1 where the dependent variable is the number of arrests in each post-demolition year and the sample is restricted to years in which the individual is between 13 and 18 years old. The point estimate for the impact on total arrests is negative, but this effect is not precisely measured ($p = 0.37$). Looking at the impact by type of arrest reveals notable heterogeneity in that the impact on property arrests is actually positive. Specifically, children displaced by demolition had 16 percent ($=0.008/0.048$) *more* property crime arrests than their non-displaced peers.⁴⁷

Finally, as discussed in Section 3, demolition has the potential to change long-run child outcomes

⁴⁵A parent in my sample is defined as any adult (age > 18 at baseline) who lives in a household with a child.

⁴⁶The LATE estimates (not reported) are similarly small in magnitude (and imprecise).

⁴⁷While the increase in property arrests during ages of peak criminal activity is somewhat surprising, supplemental analysis in Appendix Table A9 shows that there is a positive impact (reduction in arrests) in the long-run. The results in Appendix Table A9 examine arrests over the *entire* sample of post-demolition years for children. The point estimate shows that youth who relocate have 14 percent fewer arrests for violent crimes.

by affecting school quality or student achievement. Previous research by [Jacob \(2004\)](#) sheds light on this issue by providing a short-run analysis of the impact of demolition using data from the Chicago Public Schools.⁴⁸ Interestingly, he finds that displaced families are not enrolled in better schools after demolition, and there is no detectable impact on test scores or grades. Yet, there is a 9 percent increase in the probability of dropping out of high-school for older children in his sample. This suggests that the labor market benefits for older (age > 13) found in this paper are not due to improvements in schooling.

8 Multiple Comparisons

One particular concern for my results is how to manage the risk of both false positives and false negatives given that my analysis considers labor supply and additional outcomes such as social assistance usage and youth crime. I follow current best practices to adjust per-comparison p -values to account for multiple outcomes ([Jacob et al., 2015](#); [Anderson, 2008](#)). To start, I specify a limited set of outcomes for my main, confirmatory analysis. Based on the preceding analysis, I focus on four outcomes: (1) labor market participation; (2) annual earnings; (3) use of social assistance (i.e., AFDC/TANF, Foodstamps or Medicaid); and (4) total number of criminal arrests. Next, I use a two-step procedure from [Benjamini et al. \(2006\)](#) to calculate adjusted p -values that control for the false discovery rate (FDR), which is the proportion of rejections that are false positives (Type I errors).

Reassuringly, the results in Appendix Table A5 show that the main conclusions of my demolition analysis do not change based on examination of adjusted p -values to account for testing multiple outcomes. For convenience, Columns (1) and (2) repeat the results from Tables 3, 5 and 7. Column (3) reports the standard per-comparison (pairwise) p -values associated with each of the four outcomes. Again, these results show that I can reject the null hypothesis of no effect on labor market participation and earnings at the 1 percent level. Similarly, the adjusted p -values in Column (3) show that I can still reject the null of no effects even after accounting for the fact that my analysis of the impact of demolition considered four different outcomes.

⁴⁸[Jacob \(2004\)](#) has data on student outcomes up to five years following demolition and relocation.

9 Reconciling Estimates Across Studies

The positive impact of public housing demolition on labor market outcomes of children seems at odds with the results from the MTO evaluation, which is one of the most credible studies of neighborhood effects. This section begins with a description of the MTO study and reviews its findings. Next, I provide a heterogeneous treatment effects framework which explains the difference between the parameters identified in the present study and MTO. The framework highlights that the effects from MTO may be different from the average treatment effect because a relatively small fraction of families in MTO’s target population volunteered for the experiment. Estimates from public housing demolition represent effects for a more general population because families had no ability to control whether demolition affected them. In short, volunteers for MTO may differ from the sample of families from demolished housing. The final part of this section provides evidence in support of this interpretation by examining a housing voucher lottery held in Chicago just after the housing demolitions began.

9.1 The MTO Evaluation and Comparing Estimates

The MTO evaluation sought to relocate families living in public housing projects located in extremely poor neighborhoods using housing vouchers. The program operated from 1994 to 1998 and recruited 4,600 households into the experiment across five major U.S. cities. The program randomly assigned each family to one of three groups: (1) a control group that received no vouchers through MTO; (2) a treatment group that received housing vouchers that could be used to subsidize private market housing; and (3) a treatment group that received housing vouchers that could be used only to lease private market housing in Census tracts with poverty rates below 10 percent ([Sanbonmatsu et al., 2011](#)).

Using this random assignment, the MTO studies report intent-to-treat (ITT) effects of receiving a housing voucher offer and treatment-on-the-treated (TOT) effects. As explained in [Kling et al. \(2005\)](#), the latter provide an estimate of the causal effects of vouchers among those who used a voucher to move. Specifically, the TOT estimate is equal to the ITT estimate divided by the fraction of the treatment group that successfully used an MTO voucher.

Figures 6 and 7 show that there is notable contrast between estimates of the TOT effects for

each MTO treatment arm and the reduced form effect of public housing demolition. The outcomes of interest are labor market participation and annual earnings for all children who were between ages 7 and 18 at baseline in each study.⁴⁹ For each outcome, the MTO estimates are negative and fall outside of the 95-percent confidence interval around the demolition estimate. Recall that households displaced by demolition received housing vouchers so that the demolition estimate can be interpreted as an ITT effect of vouchers. In addition, the ITT estimate from the demolition sample can be interpreted as a TOT estimate if all households displaced by demolition used their voucher and none of the non-displaced households used a voucher.

9.2 A Framework for Interpreting Estimates Across Studies

Why do estimates of the impact of demolition differ from what one might have expected based on the MTO study? While families in both contexts received housing vouchers, the demolition experiment is different because use of vouchers was effectively mandatory and households had no ability to control whether they were affected by demolition. In this section, I review the importance of these differences with a formal model of causal inference in the context of vouchers and heterogeneous treatment effects.

Let V_i be an indicator of whether the family of child i used a subsidized housing voucher to lease private market housing. Our main interest is in learning about the effects of vouchers on children. Denote by Y_{1i} the outcome of child i if their family used a voucher to move, and let Y_{0i} be the outcome if their family did not use a voucher. Represent the causal effect of a voucher on child i as $\Delta_i = Y_{1i} - Y_{0i}$.

To identify the impact of a housing voucher, let O_i be a binary variable that indicates whether the family of child i received a randomly allocated housing voucher. Using the general framework of [Heckman et al. \(2001\)](#), [Angrist \(2004\)](#) and [DiNardo and Lee \(2011\)](#), I assume that the housing voucher offer O_i affects voucher use according to the following latent-index assignment mechanism,

$$V_i = \mathbf{1}(\gamma_0 + \gamma_1 O_i > \epsilon_i), \quad (4)$$

⁴⁹Note that I use the MTO estimates as reported in [Sanbonmatsu et al. \(2011\)](#) because the age of children in this sample best aligns with the ages observed in my data. Specifically, children affected by demolition are ages 21 to 32 at the end of my sample. Similarly, the sample used [Sanbonmatsu et al. \(2011\)](#) examines “grown” children who were between the ages 21 of 30 by December 1, 2007.

where ϵ_i is a random error that represents the unobserved cost of voucher use for child i 's family. The coefficient γ_1 represents the impact of receiving a voucher offer on the decision to use a voucher.

As shown in (Angrist et al., 1996), the 2SLS estimate using a randomly assigned voucher offer O_i as an instrument for voucher use (V_i) is the local average treatment effect (LATE) of vouchers,⁵⁰

$$\frac{\mathbb{E}(Y_i|O_i = 1) - \mathbb{E}(Y_i|O_i = 0)}{\mathbb{E}(V_i|O_i = 1) - \mathbb{E}(V_i|O_i = 0)} = \mathbb{E}(\Delta_i|\gamma_0 + \gamma_1 > \epsilon_i > \gamma_0). \quad (5)$$

The left side of this expression is the Wald Estimator, which is the ratio of the reduced form (ITT) effect to the difference in the voucher-use rate in the treatment and control groups. Note that this parameter is nearly identical to the TOT estimates reported in the MTO studies.⁵¹ The right-hand side is the local average treatment effect (LATE) of housing vouchers. This estimate represents the average effect of a housing voucher on children from households who “comply” by moving using a voucher that is randomly assigned to them.

With Equation 5 in mind, I consider a parametric model to illustrate how the LATE depends on the fraction of the public housing population that is induced to use a voucher within an experiment. For simplicity, I assume the distribution of (Δ_i, ϵ_i) is bivariate normal: $(\Delta_i, \epsilon_i) \sim N_2(\mu_\Delta, \mu_\epsilon, \sigma_{\Delta_i}^2, \sigma_{\epsilon_i}^2, \rho)$. Here, μ_Δ and μ_ϵ are the mean benefits to children and mean costs of using a voucher, respectively. The correlation between child gains and costs is ρ while the respective variances for benefits and costs are $\sigma_{\Delta_i}^2$ and $\sigma_{\epsilon_i}^2$, respectively.

Under these assumptions, the LATE of using a voucher is:

$$\begin{aligned} \mathbb{E}(Y_{1i} - Y_{0i}|\gamma_0 + \gamma_1 > \epsilon_i > \gamma_0) &= \mathbb{E}(\Delta_i|\gamma_0 + \gamma_1 > \epsilon_i > \gamma_0) \\ &= \underbrace{\mu_\Delta}_{\text{ATE}} + \rho\sigma_\Delta \left(\frac{\phi(\gamma_0) - \phi(\gamma_0 + \gamma_1)}{\Phi(\gamma_0 + \gamma_1) - \Phi(\gamma_0)} \right) \end{aligned} \quad (6)$$

The right side of Equation 6 shows that the LATE will differ from the average treatment effect (ATE) as a function of the parameters of Equation 4, which models whether a family uses a voucher

⁵⁰Note that the standard monotonicity assumption is implied because this latent index model in this framework has a constant coefficient for the impact of the instrument.

⁵¹MTO provides an estimate of the treatment-on the-treated (TOT), which differs from the LATE because the endogenous (first stage) variable is using an MTO voucher rather than any voucher (Kling et al., 2005). Since voucher allocation outside of MTO was limited, there are few “always-takers” who obtain a voucher even when they are assigned to the control group. This implies that $\mathbb{E}(V_i|O_i = 0) \approx 0$ and that the TOT reported in MTO is very close to the LATE of voucher use.

to move and the correlation between gains and costs (ρ). Importantly, the discrepancy between the LATE and ATE decreases with $\mathbb{P}(V_i = 1|O_i = 1) \equiv \Phi(\gamma_0 + \gamma_1)$, which is the fraction of households that use a voucher.

Figure 8 considers two different scenarios which show that the LATE may be far above or below the ATE when few households are induced to use a voucher to move. Panel (a) assumes a slight positive selection on gains ($\rho = -0.15$). This pattern is referred to as “Roy style selection,” in which parents are more likely to use a voucher if their children would experience higher benefits to moving. In this case, the LATE is much higher than the ATE if a small fraction of households use a voucher to move. Intuitively, few households have sufficiently low costs of using a voucher (ϵ_i), and these children have higher returns if $\rho < 0$. In contrast, Panel (b) illustrates a scenario where there is slight negative selection on gains ($\rho = 0.15$). With this type of selection, the children who would most benefit from moving live in households with parents who are the least likely to use a voucher. This implies that the LATE is far below the ATE when few families elect to use a voucher. Here, the children in these selected households with lower costs have less to gain if $\rho > 0$.

The key insight in this section is that the context and pattern of results in the MTO and demolition settings fit the model presented in Panel (b), which features negative selection in terms of voucher use. In the case of MTO, there was relatively low interest in using a housing voucher - that is, $P(V_i = 1|O_i = 1)$ was low. [Goering et al. \(1999\)](#) report that only 25 percent of eligible families at the five MTO sites volunteered.⁵² In addition, the point estimates and confidence intervals in MTO suggest that there are small benefits to moving for children among the households that had sufficiently low costs that they sought vouchers by signing up for the experiment. In contrast, the natural experiment created by demolition induced a broader population to use a voucher because the housing authority (essentially) randomly allocated vouchers to a large fraction of public housing residents and use of vouchers was effectively mandatory. Hence, the demolition context implies that $P(V_i = 1|O_i = 1)$ is high, and the effects in the demolition sample suggest that there are larger gains for children from families that have a relatively high cost of moving using a voucher.

⁵²In keeping with the spirit of the model shown in Equation 4, I assume that all individuals who did not volunteer for the MTO experiment know their own idiosyncratic cost. Any household with cost that exceeds $\gamma_0 + \gamma_1$ would never volunteer for the experiment and is a “never-taker” for the treatment (V_i).

9.3 The Chicago 1997 Housing Voucher Lottery

To test further for the existence of negative selection into voluntary housing mobility programs, I study a housing voucher lottery that occurred in Chicago shortly after the public housing demolition began. During the late 1990s, the Chicago Housing Authority Corporation (CHAC) created a new waiting list for Section 8 vouchers.⁵³ Because demand for these vouchers far exceeded supply, the CHAC randomly assigned vouchers to applicants.

Using data collected for [Jacob and Ludwig \(2012\)](#) and [Jacob et al. \(2015\)](#), I analyze the CHAC 1997 lottery using the following definitions for treatment and control groups.⁵⁴ There were 82,607 income-eligible applicants and the CHAC randomly assigned each a position on a voucher wait list. The top 18,110 applicants and their families comprise the treated group that received housing vouchers. Families whose lottery position was between 35,001 and 82,607 are the control group because the CHAC told these families that they would not receive vouchers.⁵⁵

Panel (a) of Table 9 shows voucher-usage statistics for my main analysis sample of children from lottery households that lived in public housing at the time of applying for the CHAC 1997 lottery.⁵⁶ The first row shows that 63 percent of the treated group uses a housing voucher, while a notable share (31 percent) of the control group also obtains a housing voucher from some program other than the CHAC 1997 lottery.⁵⁷ Similar to the MTO unrestricted treatment arm, Appendix Table A6 shows that households that used a voucher through the CHAC 1997 program moved to notably better neighborhoods. For example, winning a voucher decreased the neighborhood poverty rate by about 10 percentage points (22 percent).

Also similar to the MTO results, Panel (b) of Table 8 shows that there is no measurable impact

⁵³In 1995, the U.S. Department of Housing and Urban Development (HUD) transferred housing operations from the Chicago Housing Authority (CHA) to a non-profit organization, the Chicago Housing Authority Corporation (CHAC).

⁵⁴For a detailed description of the data including summary statistics, see Appendix Section A5.

⁵⁵The CHAC originally planned to provide voucher offers to applicants whose position on the wait list fell between 18,111 and 35,000. However, due to fiscal constraints the CHAC was unable to meet this initial plan. For more detailed background on the CHAC lottery, see [Jacob and Ludwig \(2012\)](#).

⁵⁶The lottery children from public housing are the relevant comparison for my demolition sample because winning a Section 8 voucher does not directly affect income or wage rates because the rules for vouchers and project-based housing are similar. In other words, changing the type of housing assistance does not change a household's budget set. Note that this analysis of lottery children from public housing is distinctly different from the analysis in [Jacob et al. \(2015\)](#) which focused on lottery sample children living in private market housing.

⁵⁷Previous studies have highlighted that voucher utilization rates tend to be far less than unity. For example, [Jacob and Ludwig \(2012\)](#) find that about 43 percent of households living in private market housing at baseline chose to use a voucher offer provided through the CHAC 1997 lottery. [Finkel and Buron \(2001\)](#) also study voucher take-up rates across U.S. cities and find that the overall voucher utilization rate was 69 percent in 2000.

of vouchers for children from households that volunteered for the CHAC 1997 lottery. Column (2) provides the reduced form (ITT) estimates while Column (3) provides estimates of the LATE of vouchers. Specifically, I compute the LATE estimates by estimating a 2SLS system in which the dependent variable in the first stage is an indicator for whether an individual used a voucher. The reduced form effects on the probability of employment are fairly precisely estimated zeroes, while the estimate for earnings is also small but has a larger standard error. Notably, the lottery sample 2SLS estimate for the labor market participation and earnings fall far below the lower bound on the 95-percent confidence interval around the demolition estimates (see Figure 6 and 7).

9.4 Interpretation of the CHAC 1997 Lottery Results

The relatively small estimates of the impact of vouchers detected in the CHAC 1997 lottery sample are consistent with the hypothesis that there are relatively small gains for children from households who are likely to use or seek vouchers voluntarily. However, there are other interpretations. One concern is that the treatment effects may be correlated with observed characteristics that differ between the demolition and lottery samples.⁵⁸ For example, Appendix Table A7 shows that households in the demolition sample are much more disadvantaged across a number of important characteristics such as adult labor market activity and criminal history.

To address this difference, I re-weight the demolition sample to be more similar to the more-advantaged CHAC 1997 lottery sample. Specifically, I pool children in the demolition and lottery samples and estimate a propensity score where the dependent variable is an indicator for membership in the lottery sample. The covariates include a rich set of observable household and child characteristics including baseline measures such as adult labor market activity and criminal behavior.⁵⁹ As shown in Column (4) of Table A7, re-weighting using these propensity scores insures that the characteristics of the weighted demolition sample are statistically indistinguishable from the lottery sample.

Panel (c) of Table 8 shows that even after re-weighting the demolition estimates are still strikingly different from the lottery sample results. Moreover, the weighted sample point estimates are

⁵⁸This can occur under a potential outcomes model where $Y_{ji} = \mu_j(X_i) + U_j$ for $j \in \{0, 1\}$ where $\mu_j(X_i)$ is a general function of observables X_i and U_j represents unobservable determinants of potential outcomes (Heckman et al., 2001).

⁵⁹Appendix Figure A2 provides further details on the propensity score estimates and illustrates the propensity score distributions for households in the lottery and demolition samples, respectively.

similar to the results for the un-weighted analysis. This demonstrates that differences in observed characteristics are unlikely to explain why the impact of moving is larger in the demolition sample. Moreover, it also suggests that observed characteristics are not correlated with unobservable (costs) that drive the decision to move.

Overall, this evidence suggests that large gains among children from households that are unlikely to use or seek vouchers drive the effects detected in the demolition sample. The point estimates and confidence intervals for labor market participation in the MTO and CHAC 1997 lottery samples suggest that children from volunteer households have relatively smaller gains. As shown in Figure 8, this pattern of negative selection between the probability that a parent decides to move using a voucher and the gains to children is consistent with “reverse Roy” selection where children with the most to gain from moving have parents who are the *least* likely to move using a voucher.

One explanation for this negative selection is that parents who invest more in human capital or other margins may be the most willing or able to move out of public housing. In addition, moving to a neighborhood with lower poverty and less crime may substitute for parenting effort or household resources. Interestingly, supplementary research of the MTO evaluation provides suggestive evidence that parents living in public housing do compensate and substitute parenting effort in response to concerns over neighborhood safety. Specifically, [Kling et al. \(2001\)](#) report that moving through MTO actually reduced parents’ child monitoring intensity, presumably because treated parents felt safer in their new neighborhood.

To provide further intuition for why reverse Roy selection could occur in the context of a housing mobility program, Appendix A6 provides a stylized model in which neighborhood safety and parenting effort (e.g., child monitoring) are substitutes in the production of child outcomes. In the model, parents living in the same public housing project have different perceptions about neighborhood safety while having identical preferences and resources. As expected, parents who have the worst view of their neighborhood’s safety opt into a program that provides vouchers. In an experimental setting, these parents will choose high parenting effort when they are randomized into the control group, and this reduces the contrast between treatment and control child outcomes. Intuitively, the model predicts larger treatment effects for children of parents who would *not* seek vouchers because these non-volunteering parents have relatively more optimistic perception of safety, and this view causes them to choose lower parenting (child monitoring) effort.

Finally, it is worth highlighting that previous studies have found a similar pattern of negative selection in education markets. [Walters \(2014\)](#) and [Hastings et al. \(2008\)](#) examine school choice and academic performance in Boston and North Carolina, finding that children who would have the highest gains from attending charters or other high-quality schools are the least likely to seek these schooling options. Similarly, [Kline and Walters \(2014\)](#) find that children who would have the largest test score gains from enrolling in Head Start have a lower likelihood of participating.

10 Cost-Benefit Analysis

This section uses the results from Section 6 to provide back-of-the-envelope estimates of the benefits and costs of relocating youth from project-based public housing assistance using unrestricted Section 8 vouchers. These calculations are informative because we know little about the optimal design of housing assistance ([Olsen, 2003](#)). Moreover, the fact that the U.S. federal government currently spends \$46 billion on housing assistance underscores the need for a comparison of the relative efficiency of different housing assistance programs ([Collinson et al., 2015](#)).⁶⁰

To understand the earnings benefit of relocation, I use the reduced form estimate from Table 3. Recall from Section 5 that these estimates reflect the impact of relocating using a subsidized Section 8 housing voucher relative to the counterfactual of living in project-based public housing. With this in mind, the estimates show that replacing project-based assistance increased youth earnings by about \$602, a 16 percent increase above the control group mean.

Similar to [Chetty et al. \(2015\)](#), I translate the impact of relocation into a predicted lifetime impact on income using the following assumptions: (1) the 16 percent increase in annual income is constant over the lifecycle; (2) the lifecycle profile of income for demolition participants follows the U.S. population average; (3) the real wage growth rate is 0.5 percent; and (4) the discount rate is 3 percent, approximately the current 30-year Treasury bond rate. Under these assumptions, moving youth out of public housing using vouchers would increase pre-tax lifetime income by about \$45,000. The present value of this increase in lifetime income is about \$12,000.⁶¹ For a family with

⁶⁰The \$46 billion in expenditures on housing programs is more than twice the level of federal spending on cash welfare and more than five times the amount spent on Head Start ([Collinson et al., 2015](#)).

⁶¹The estimate of the impact on lifetime income is calculated as follows. First, I calculate the mean of individual pre-tax annual income for all working-age adults (age 19 to 65) from the 2000 Census. Next, I apply a 0.5 percent wage growth rate, which yields an undiscounted sum of lifetime earnings for the average American at \$1.75 million. Average income for non-displaced (control) youth in my demolition sample is about 16 percent of the average adult

two children, this corresponds to a total family benefit of about \$24,000.

In terms of cost, a variety of previous studies suggests that the direct fiscal cost of housing voucher programs is much lower than the cost of project-based housing assistance ([Olsen, 2014](#)). This implies that housing authority payments for moving expenses were the main direct cost of replacing project-based assistance with Section 8 vouchers.⁶² To the best of my knowledge, there is no record on moving payments provided by the Chicago Housing Authority. However, one way to get a sense of these costs is to look at the federal moving-cost fixed payment schedule that the U.S. federal government uses to reimburse individuals displaced by government projects such as highway construction. The fixed moving cost payment is set at \$1,100 for a furnished four bedroom apartment in the state of Illinois.⁶³

Overall, this simple accounting suggests that relocating children from public housing is likely to generate a high rate of return on investment since the value of increased lifetime earnings is about \$24,000 for a family with two children and the main cost comes from moving expenses which are most likely around \$1,100 per family. Assuming that there is just a 10 percent increase in tax revenue for relocated children, this implies that the government would save about \$1,300 ($= \$24,000 \times 0.10 - \$1,100$) per family. At the same time, it is important to recognize that these cost-benefit calculations ignore any *negative* spillover effects on residents of neighborhoods where displaced households move. In a neighborhood-level study of demolition and crime, [Aliprantis and Hartley \(2015\)](#) found no detectable increase in violent crime for neighborhoods that received displaced individuals. However, there were detectable increases in other types of crime such as assaults and property crime.

11 Conclusion

This paper exploits public housing demolition as a natural experiment to study the impact of growing up in a disadvantaged neighborhood. During the 1990s, the CHA selected some public housing buildings for demolition while leaving other buildings untouched. This event provides quasi-

in the U.S. This implies that the estimated effect of relocation on pre-tax undiscounted lifetime earnings is about \$45,000 ($= 0.16 \times 0.16 \times \$1.75m$). Note that all monetary values are in 2012 dollars.

⁶²Unlike households in the MTO study, there were few supplemental services provided to families forced to relocate due to building demolition ([Jacob, 2004](#)).

⁶³The fixed moving payment is determined by the number of furnished bedrooms and varies by state.

random variation in childhood environment because authorities selected buildings for demolition for reasons unrelated to resident characteristics.

Importantly, the setting in this study provides a unique opportunity to study public housing families who were not volunteers and were required to relocate. This contrasts with the well-known MTO experiment that provided volunteering families with the option of leaving public housing using vouchers. This optional feature of the MTO treatment is notable because 75 percent of the (eligible) population targeted by MTO chose not to volunteer for the experiment. While the MTO evaluation cannot speak to the effects of relocation for these “never-takers”, there is no such compliance issue in the demolition sample because nearly all displaced households relocate from public housing.

My analysis reveals that the benefits of relocation are more general than what one might expect given estimates from MTO. I find that children displaced by public housing demolition have notably better adult labor market outcomes compared to their non-displaced peers. This positive impact is detectable regardless of the age at which a child’s family relocates. This contrasts with analysis from MTO, which finds a positive impact only for children who were young when their families moved ([Chetty et al., 2015](#)).

One of the main contributions of this paper is to provide a new explanation to reconcile findings in the neighborhood effects literature. On the one hand, a number of observational studies find a substantial positive correlation between child outcomes and measures of neighborhood quality ([Galster et al., 2007](#); [Ellen and Turner, 1997](#)). This pattern inspired a large theoretical literature to support a causal interpretation of these estimates (e.g., [Massey and Denton \(1993\)](#); [Wilson \(1987\)](#)). On the other hand, some have suggested that neighborhood effects must be small in light of the lack of detectable improvement in outcomes for some children who moved through the MTO experiment.

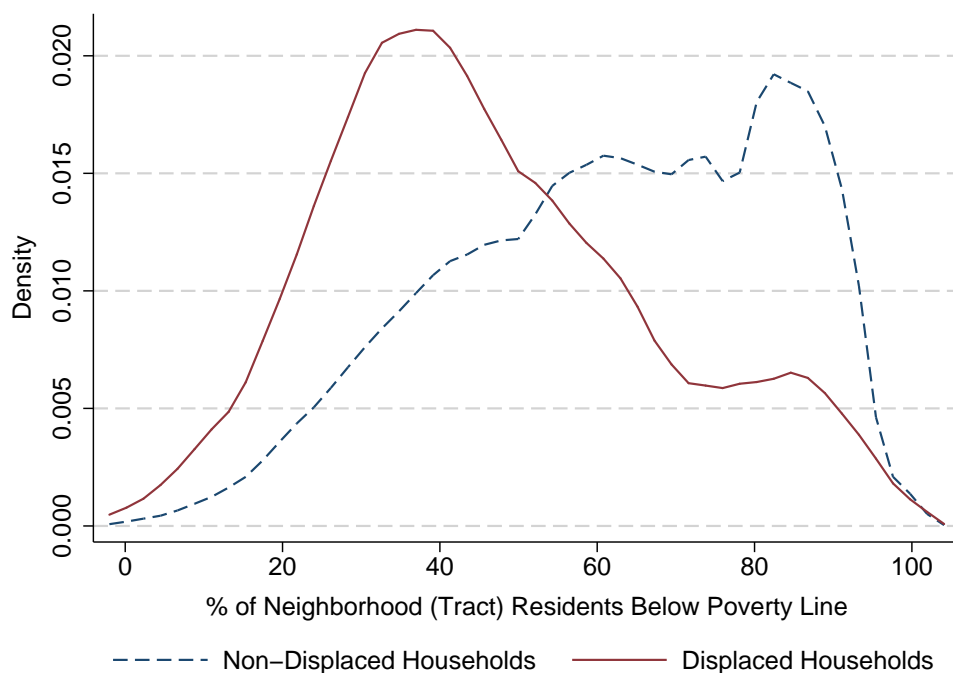
One explanation for the discrepancy between observational studies and MTO is that children whose families move through voluntary mobility programs are negatively selected on the potential gains to treatment (moving using a voucher). The lack of effects in [Sanbonmatsu et al. \(2011\)](#) suggests that the large benefits of relocation detected in the present paper are driven by children whose households had relatively low demand for moving out of public housing. This interpretation is further supported by my analysis of the CHAC 1997 housing voucher lottery that also occurred

in Chicago after the beginning of the demolitions. Similar to MTO, there are no detectable benefits of moving using a voucher for children from households that volunteered and won the CHAC 1997 lottery. The contrast in effects persists even after re-weighting the demolition sample to match the observed characteristics in the lottery sample.

In terms of housing policy, this paper demonstrates that relocation of low-income families from distressed public housing has substantial benefits for both children (of any age) and government expenditures. I estimate that moving a child out of public housing using a standard housing voucher would increase total lifetime earnings by about \$45,000, which has an equivalent present value of \$12,000. In all likelihood, this will yield a net gain for government budgets because there are negligible moving costs to relocating families and housing vouchers have similar costs compared to project-based assistance.

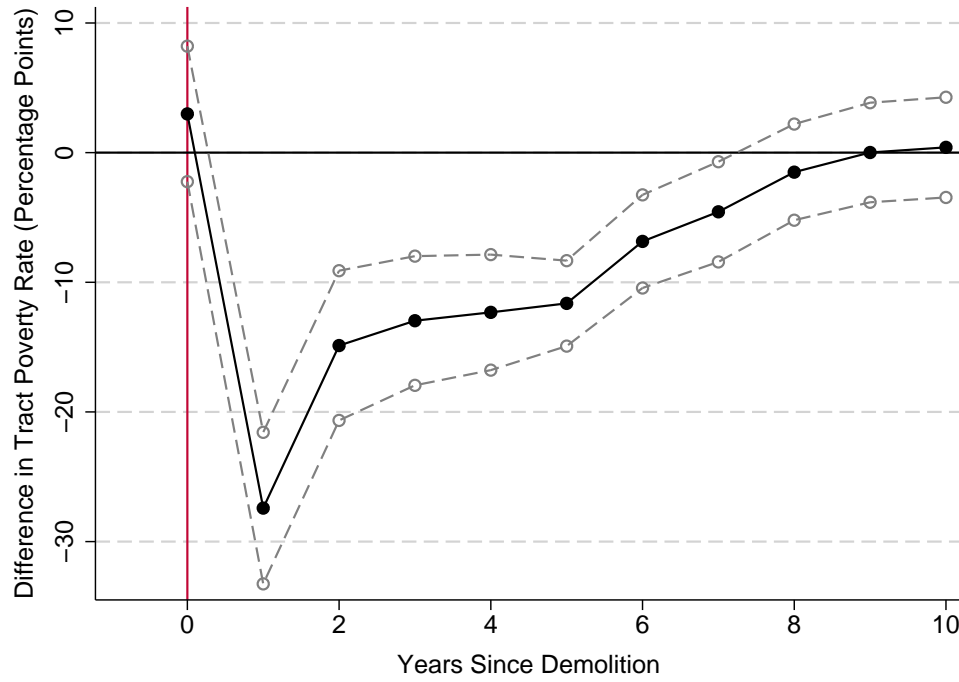
12 Figures and Tables

Figure 1: Density of Neighborhood Poverty for Displaced (Treated) and Non-displaced (Control) Households



Notes: This figure displays the density of the Census tract-level poverty rate for households ($N = 2,767$) with at least one child (age 7 to 18 at baseline) affected by demolition. Poverty rates for each household are duration-weighted averages over all locations that a household lived since being displaced (treated) by housing demolition. Household location is tracked to 2009. The duration-weighted poverty rate for households that were displaced by demolition is shown in the solid red line, while households from non-demolished buildings are shown in the dashed blue line.

Figure 2: Difference in Neighborhood Poverty For Displaced and Non-displaced Households by Post-Demolition Year



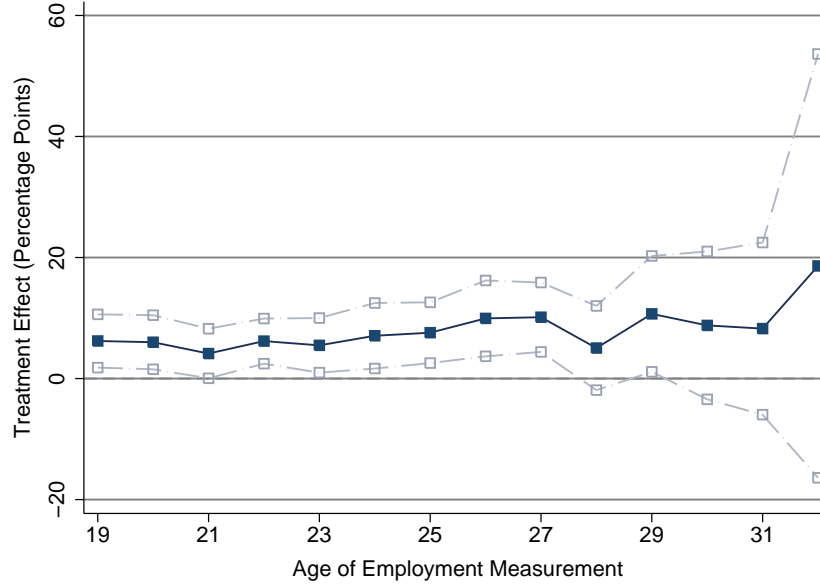
Notes: This figure illustrates the change over time in the difference in neighborhood poverty rate between displaced (treated) and non-displaced (control) households with children (age 7 to 18 at baseline). Specifically, I plot (in solid black) the set of coefficients π_y for $y \in \{0, \dots, 10\}$ from the following specification:

$$pbpov_{htp} = \sum_{y=0}^{y=10} \pi_y treat_h \mathbf{1}(t - t^* = y) + \sum_{y=0}^{y=10} \delta_y \mathbf{1}(t - t^* = y) + \psi_p + \epsilon_{ht}$$

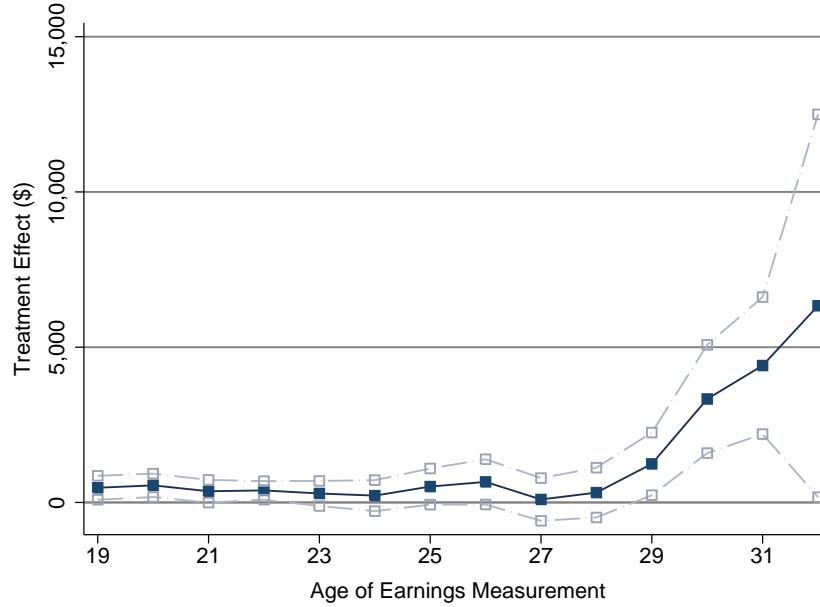
where h indexes a household; t represents years; and p indexes projects. The dependent variable is the percentage of residents living below the poverty line in a Census tract and ψ_p is a set of project fixed effects. The variable t^* represents the year of demolition for a particular household. Recall that public housing demolitions occur from 1995-1998 in my sample. The variable $treat_h$ is an indicator for treatment (displaced) status. The data used with this specification is a panel for a particular household where the first observation is the poverty rate based on the household's address at the time of demolition (t^*). Hence, the set of coefficients π_y represent the difference in poverty rate between displaced (treated) and non-displaced (control) households in a particular post demolition period (y). There are 2,767 households in the sample. The dashed gray lines in the figure also outline the 95-percent confidence interval for the year-specific point estimates.

Figure 3: Labor-Market Treatment Effects for All Children by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings (\$)



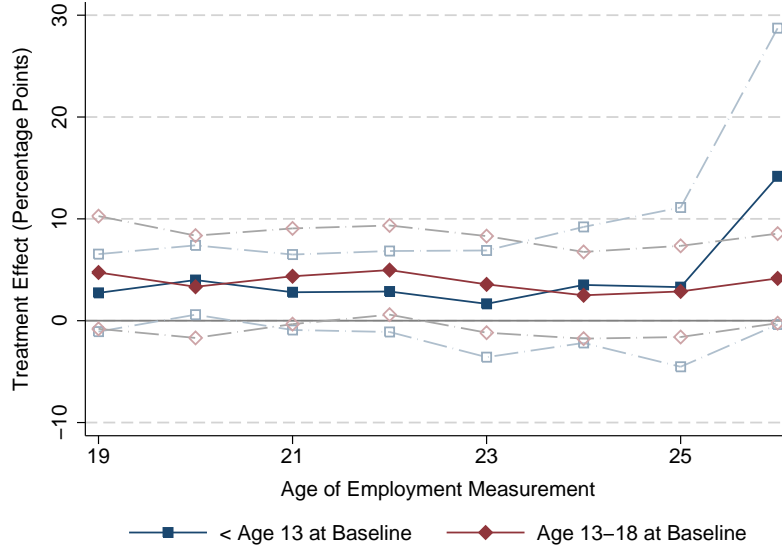
Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children (age 7 to 18 at the time of demolition). Specifically, each point (red circles) is a coefficient $\alpha_j \forall j \in \{19, \dots, 32\}$ from the following specification:

$$y_{itp} = \sum_{j=19}^{32} \alpha_j D_{i,b} \mathbf{1}(\text{age}_{i,t} = j) + X_i' \theta + \psi_p + \delta_t + \epsilon_{itp}$$

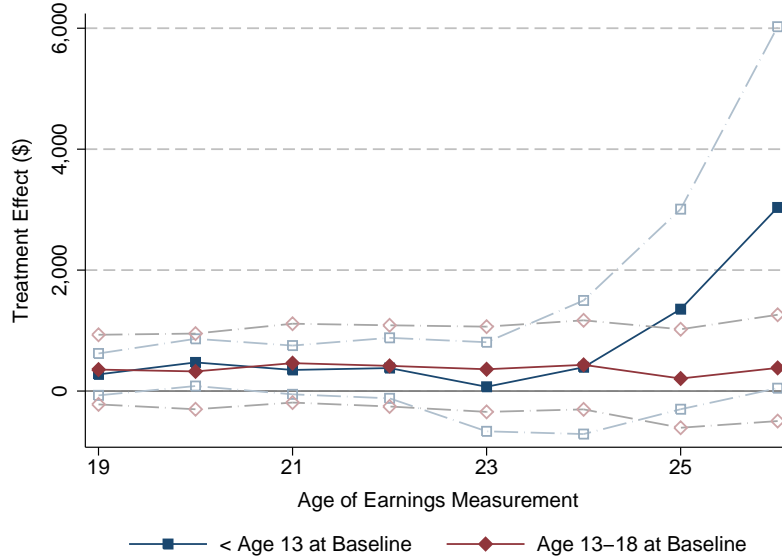
where i is an individual; t indexes years; and p is a project. The terms δ_t and ψ_p are year and project fixed effects, respectively. The dummy variable D takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the vector X_i . The area shaded in blue that surrounds the plotted coefficients covers the 95-percent confidence interval for the age-specific point estimates. Note that age 32 is the last age observed for the oldest children in the sample. All monetary units are in 2012 dollars.

Figure 4: Younger vs Older Children: Labor-Market Treatment Effects by Age of Measurement

(a) Dependent Variable: Employed (=1)



(b) Dependent Variable: Annual Earnings (\$)

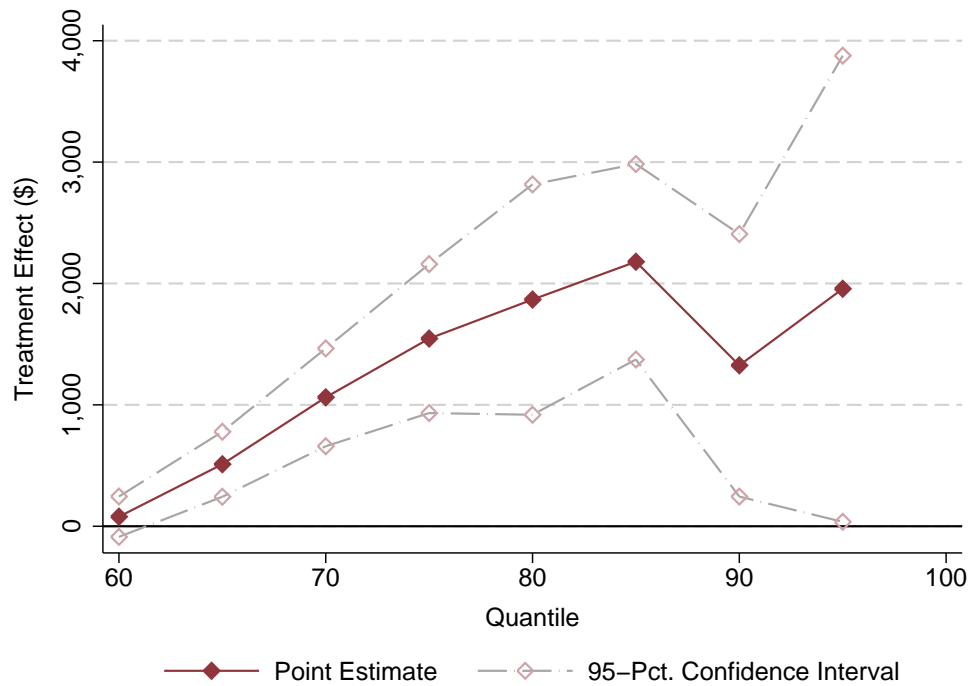


Notes: The figures in Panels (a) and (b) display estimated treatment effects at adult ages for children affected by demolition. Each figure presents two sets of results: estimates for younger (age < 13 at baseline) and older (age 13 to 18 at baseline) children as solid red diamonds and solid blue squares, respectively. Each point plotted on the figure is a coefficient $\alpha_j \forall j \in \{19, \dots, 26\}$ from the following specification:

$$y_{itp} = \sum_{j=19}^{26} \alpha_j D_{i,b} \mathbf{1}(\text{age}_{i,t} = j) + X_i' \theta + \psi_p + \delta_t + \epsilon_{itp}$$

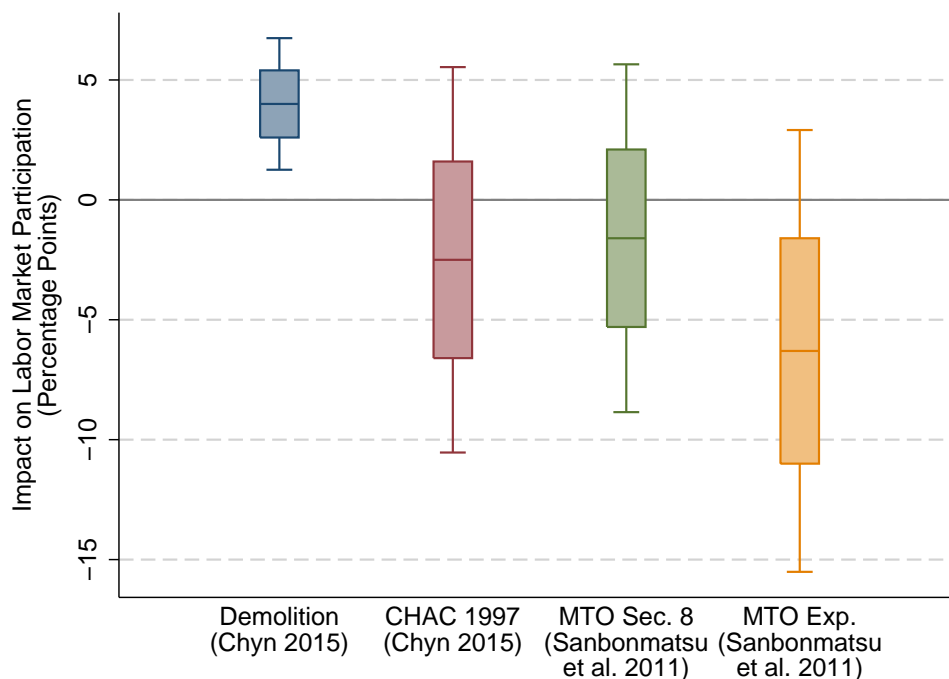
where i is an individual; t indexes years; and p is a project. The terms δ_t and ψ_p are year and project fixed effects, respectively. The dummy variable D takes a value of one if an individual was displaced by demolition. The main effects for the indicator terms for individual age are included in the vector X_i . Note that the index only goes up to age 26 because this is the oldest age of measurement observed for younger children (age < 13 at baseline). The dashed lines in the figures plot out the 95-confidence intervals for each age-specific point estimate. All monetary units are in 2012 dollars.

Figure 5: Quantile Treatment Effects for Adult Earnings of Children



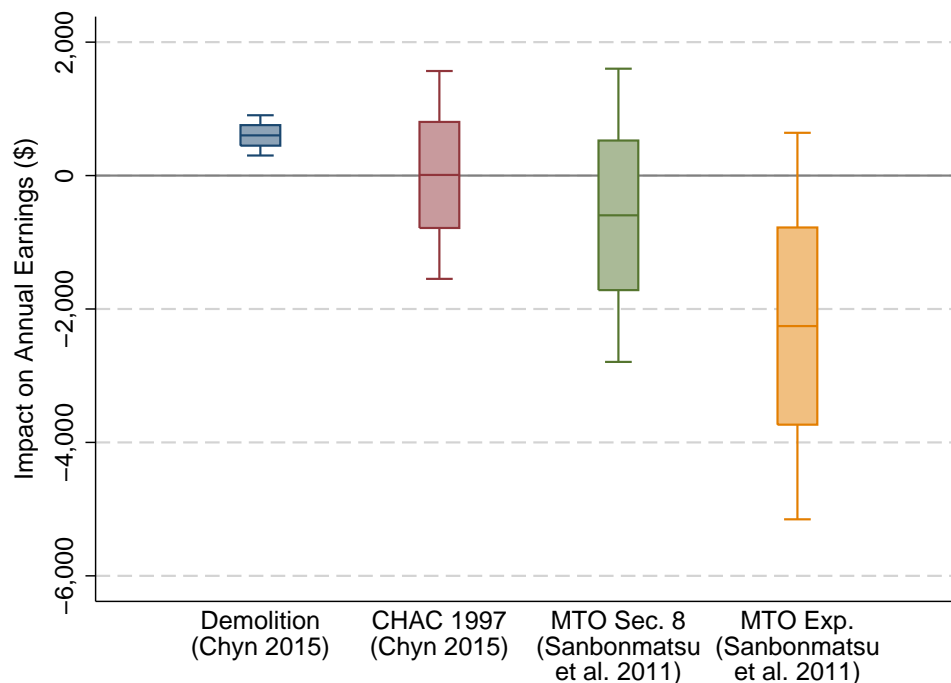
Notes: This figure plots estimates of the quantile treatment effect on adult earnings outcomes for children (age 7 to 18 at baseline) affected by public housing demolition. These estimates measure the treatment effect for particular percentiles of the distribution of earnings. In other words, the quantile treatment effect estimate for the 60th percentile measures the difference between the 60th percentile of the treated (displaced) and control (non-displaced) earnings distributions. The bars surrounding each point estimate are the 95-percent confidence interval. Note that the lower bound of the x -axis on the figure is restricted to the 60th percentile because a large fraction of earnings are equal to zero.

Figure 6: Effects on Adult Employment of Children Across Studies



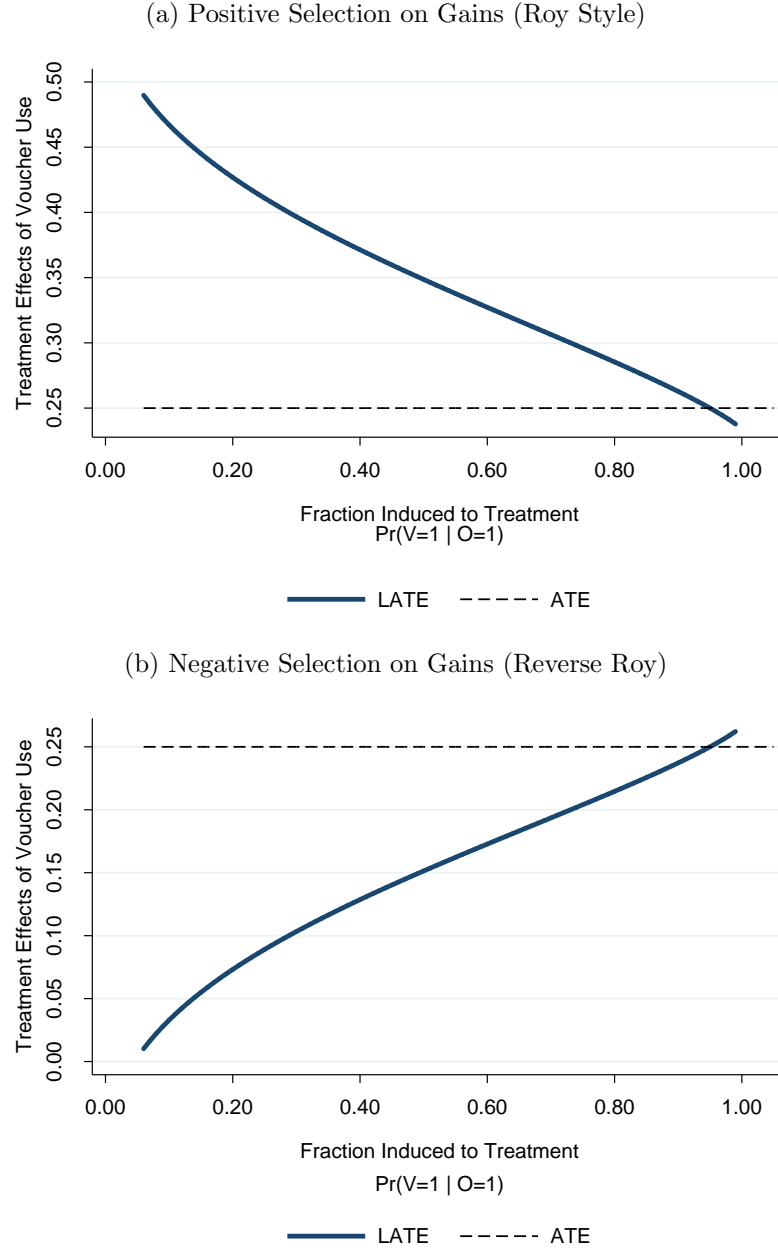
Notes: This figure displays box and whisker plots of the effects on adult labor market participation for children (age 7 to 18 at baseline) from different studies. The center of each box is the point estimate in each sample while the top and bottom of each box represent effects that are one standard error above and below the point estimate. The top and the bottom of the whiskers display the 95-percent confidence interval. The MTO evaluation provided vouchers to low-income households, and this figure reports the treatment-on-the-treated effects (TOT) of vouchers as reported in [Sanbonmatsu et al. \(2011\)](#). Similarly, households received vouchers if they were displaced by public housing demolition or if they won the CHAC 1997 housing lottery. The demolition results are the intent-to-treat (ITT) effect which is equal to TOT effect of a voucher if all households displaced by demolition used their voucher offer and none of the non-displaced households used a voucher. The CHAC lottery result is the local average treatment effect (LATE) which is similar to a TOT because only a small fraction of the control group received vouchers even if they did not win the CHAC 1997 lottery. The demolition and CHAC lottery samples contain 5,246 and 4,661 children, respectively. The MTO final evaluation has 3,052 children in the sample for the labor market outcome analysis.

Figure 7: Effects on Adult Earnings of Children Across Studies



Notes: This figure displays box and whisker plots of the effects on adult earnings outcomes for children (age 7 to 18 at baseline) from different studies. The center of each box is the point estimate in each sample while the top and bottom of each box represent effects that are one standard error above and below the point estimate. The top and the bottom of the whiskers display the 95-percent confidence interval. The MTO evaluation provided vouchers to low-income households, and this figure reports the treatment-on-the-treated effects (TOT) of vouchers as reported in [Sanbonmatsu et al. \(2011\)](#). Similarly, households received vouchers if they were displaced by public housing demolition or if they won the CHAC 1997 housing lottery. The demolition results are the intent-to-treat (ITT) effect which is equal to TOT effect of a voucher if all households displaced by demolition used their voucher offer and none of the non-displaced households used a voucher. The CHAC lottery result is the local average treatment effect (LATE) which is similar to a TOT because only a small fraction of the control group received vouchers even if they did not win the CHAC 1997 lottery. The demolition and CHAC lottery samples contain 5,246 and 4,661 children, respectively. The MTO final evaluation has 3,052 children in the sample for the labor market outcome analysis.

Figure 8: The Local Average Treatment Effect When the Fraction Induced to Treatment Varies



Notes: Panels (a) and (b) display the local average treatment effect (LATE) as a function of the fraction of the population that is induced to use a housing voucher. Panel (a) is drawn so that there is positive selection on gains ($\rho = -0.15$), and treatment effects are largest for individuals who are most likely to take the treatment. Panel (b) is drawn so that there is negative selection on gains ($\rho = 0.15$), and treatment effects are larger for individuals who are less likely to take the treatment. The x -axis shows the fraction of the public housing population that is induced to use a voucher ($\mathbb{P}(V = 1 | O = 1)$). The models depicted in each panel show how the effects of vouchers in the demolition setting differ from findings from the MTO evaluation. In the MTO context, few families within the targeted public housing population were induced to use a voucher. In the demolition context, the fraction of families that used vouchers was higher because the housing authority randomly selected buildings for demolition and voucher-use was effectively mandatory for displaced families. See Section 9 for further details and discussion. Other parameters are set to the following values in both panels: $\sigma_\epsilon = 0.15$, $\mu_\Delta = 0.25$ and $\gamma_0 = 0.05$.

Table 1: Comparison of Displaced (Treated) and Non-displaced (Control) Children and Adults at Baseline (Prior to Demolition)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Children		Male Children		Female Children		Adults	
	Control Mean	Difference: Treated– Control, Within Estimate	Control Mean	Difference: Treated– Control, Within Estimate	Control Mean	Difference: Treated– Control, Within Estimate	Control Mean	Difference: Treated– Control, Within Estimate
Demographics								
Age	11.714	0.035 [0.159]	11.548	0.145 [0.196]	11.873	-0.070 [0.186]	28.851	0.810** [0.312]
Male (=1)	0.489	-0.008 [0.017]					0.128	-0.001 [0.011]
Past Arrests (#)								
Violent	0.015	0.005 [0.007]	0.028	0.011 [0.014]	0.004	-0.003 [0.009]	0.185	-0.017 [0.032]
Property	0.011	0.010 [0.009]	0.018	0.015 [0.014]	0.004	0.004 [0.010]	0.156	0.016 [0.020]
Drugs	0.025	0.000 [0.013]	0.054	0.017 [0.023]	0.000	-0.018 [0.012]	0.166	0.031 [0.022]
Other	0.022	0.004 [0.008]	0.035	0.015 [0.014]	0.011	-0.008 [0.008]	0.230	-0.018 [0.028]
School Outcomes†								
Old for Grade (=1)	0.197	-0.013 [0.012]						
Reading Score (Rank)	22.800	-0.400 [1.070]						
Math Score (Rank)	25.100	-0.730 [1.530]						
Economic Activity††								
Employed (=1)							0.173	0.006 [0.016]
Earnings							\$1,493.75	-\$45.91 [193.358]
N (Individuals)		5,250		2,547		2,703		4,331

Notes: Children are age 7 to 18 in the year prior to demolition (baseline) while adults are over age 18. The control mean statistics – Columns (1), (3), (5) and (7) – refer to the averages for non-displaced individuals. For each outcome (row), I compute the difference between displaced and non-displaced individuals by regressing the individual-level outcome on an indicator for treatment (displaced) status and a set of project fixed effects. The coefficient for the treatment indicator is reported in Columns (2), (4), (6) and (8). Standard errors are presented below each regression estimate and are clustered at the public housing building level. Note that statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. † These schooling statistics come from Jacob (2004) which examined children from a nearly identical sample of public housing projects subject to building demolition. †† The administrative data on employment begins in the first quarter of 1995. For individuals who experience a demolition in 1995, I use this first quarter of earnings (scaled to an annual figure) as the baseline measure because this first quarter precedes any demolition activity.

Table 2: Impact of Demolition on Neighborhood Characteristics

	(1)	(2)	(3)	(4)
	3 Years After Demolition		8 Years After Demolition	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Household Has Address (=1)	0.777 [0.020]	0.01	0.656 [0.020]	0.007
<i>Restricted to Households with Address</i>				
Public Housing Address (=1)	0.549 [0.042]	-0.440**	0.105 [0.033]	-0.058*
Distance from Baseline Address (miles)	1.993 [0.458]	1.409**	4.914 [0.419]	-0.447
Tract Characteristics:				
Black (%)	94.897 [1.125]	-2.563**	90.042 [1.230]	-0.82
Below Poverty (%)	64.208 [2.531]	-12.929**	40.858 [1.884]	-1.571
On Welfare (%)	57.153 [2.164]	-18.365**	34.821 [1.673]	-1.392
Unemployed (%)	39.337 [1.497]	-12.422**	24.852 [1.156]	-2.363**
Violent Crime (per 10,000)	68.855 [4.371]	-23.426**	30.801 [2.547]	3.236
Property Crime (per 10,000)	103.247 [10.122]	-15.72	68.675 [4.269]	5.902
N (Households)		2,767		2,767
N (Households with Address)		2,162		1,824

Notes: The unit of analysis in this table is a household with at least one child (age 7 to 18 at baseline). The table shows the analysis of location and neighborhood characteristics in the years following public housing demolition for displaced and non-displaced households. Location is measured using address data from IDHS social assistance case files. Some households lack address data in the years following demolition because they are not on active social assistance cases. The control mean statistics in Columns (1) and (3) refer to averages for non-displaced households. The mean difference between displaced and non-displaced households three years after building demolition is reported in Column (2). This difference is computed from a regression model where the dependent variable is a household location outcome (each row) measured three years after demolition and the independent variables include an indicator for treatment (displaced) status and a set of project fixed effects. Column (4) reports similar results where the location outcome is measured eight years after demolition. Robust standard errors are clustered at the public housing building level. Neighborhood measures such as percent black, percent of residents below poverty and percent on public assistance are measured at the tract level using Census 2000 data. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 3: Impact of Demolition on Adult Labor Market Outcomes of Children

	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.419	0.040*** [0.014]
Employed Full Time (=1)	0.099	0.013** [0.006]
Earnings	\$3,713.00	\$602.27*** [153.915]
Earnings (> 0)	\$8,856.91	\$587.56** [222.595]
N (Obs.)		35,382
N (Individuals)		5,246

Notes: This table analyzes adult labor market outcomes for displaced (treated) and non-displaced (control) children (age 7 to 18 at baseline). The control mean statistic – Column (1) – refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. See Equation 1 of the text for more details. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. Note that the analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table 4: Impact of Demolition on Adult Labor Market Outcomes of Children By Sex

	Panel Model Results			
	(1)	(2)	(3)	(4)
	Males		Females	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Employed (=1)	0.325	0.017 [0.019]	0.505	0.066*** [0.014]
Employed Full Time (=1)	0.080	0.013 [0.008]	0.117	0.015* [0.008]
Earnings	\$2,946.51	\$417.46* [236.705]	\$4,416.94	\$806.22*** [188.520]
Earnings (>0)	\$9,55.43	\$552.21 [439.299]	\$8,739.53	\$609.26** [274.111]
N (Obs.)		16,876		18,506
N (Individuals)		2,546		2,700

Notes: This table analyzes adult labor market outcomes for displaced (treated) and non-displaced (control) children (age 7 to 18 at baseline) by sex. The control mean statistics – Columns (1) and (3) – refer to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Column (2) for males and in Column (4) for females. This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. The analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table 5: Impact of Demolition on Adult Public Assistance Usage of Children

	Panel Model Results					
	(1)	(2)	(3)	(4)	(5)	(6)
	All		Males		Females	
	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate	Control Mean	Difference: Treated–Control, Within Estimate
Any Assistance	0.630	0.013 [0.022]	0.502	0.020 [0.028]	0.746	0.012 [0.025]
Foodstamps	0.509	-0.001 [0.021]	0.349	0.006 [0.023]	0.656	0.000 [0.026]
Medicaid	0.477	0.005 [0.021]	0.307	0.015 [0.025]	0.633	0.002 [0.025]
TANF	0.123	-0.001 [0.010]	0.022	0.005 [0.008]	0.216	-0.002 [0.014]
N (Obs.)		35,532		16,928		18,604
N (Individuals)		5,250		2,547		2,703

Notes: This table analyzes adult public assistance utilization for displaced and non-displaced children (age 7 to 18 at baseline). The control mean statistics – Columns (1), (3) and (5) – refer to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where an assistance outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression include an indicator for displaced (treated) status and a set of project fixed effects. See Equation 1 of the text for more details. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 6: Impact on Labor Market Outcomes of Parents

	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.489	0.004 [0.015]
Employed Full Time (=1)	0.192	0.015 [0.013]
Earnings	\$6,281.49	\$403.76 [335.892]
Earnings (>0)	\$12,836.39	\$783.19 [478.826]
N (Obs.)		52,028
N (Individuals)		4,077

Notes: This table analyzes labor market outcomes for displaced and non-displaced parents defined as adults (age > 18 at baseline) living in households with children affected by demolition. The control mean statistic – Column (1) – refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced households is reported in Column (2). This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. See Equation 1 of the text for more details. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 7: Impact on Adolescent Criminal Activity of Children

	(1)	(2)	(3)	(4)
	All Children (Age 7 to 18 at Baseline)			
	Control Mean	Difference: Treated–Control, Within Estimate	N (Obs.)	N (Individuals)
Adolescent Age Criminal Arrests				
Total	0.369	-0.022 [0.024]	21,097	4,917
Violent	0.086	-0.005 [0.007]	21,097	4,917
Property	0.048	0.008* [0.004]	21,097	4,917
Drugs	0.106	-0.012 [0.011]	21,097	4,917
Other	0.129	-0.013 [0.011]	21,097	4,917

Notes: This table analyzes criminal activity for children (age 7 to 18 at the time of demolition). Note that the sample is restricted to post-demolitions observations where children are between ages 13 to 18 (adolescent ages). This implies that the very oldest children (by baseline age) are excluded from this analysis. The control mean statistic in Column (1) refers to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where an outcome (each row) is the dependent variable for individual i in year t . Note that the panel for each individual is restricted to the years after demolition. The independent variables in the regression include an indicator for treatment (displaced) status and a set of project fixed effects. Robust standard errors are clustered by at the public housing building level. Statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table 8: CHAC 1997 Voucher Lottery and Re-Weighted Demolition Analysis

(a) Voucher Use and Mobility Analysis				
	(1)	(2)	(3)	(4)
	Control Mean	Treated Mean	<i>p</i> -value Difference: Treated– Control	N
Leased Using CHAC 1997 Voucher	0.00	0.49	0.00	4,702
Leased Using Any Voucher	0.31	0.63	0.00	4,702
(b) Lottery Sample: Effect of Vouchers				
	(1)	(2)	(3)	(4)
	Control Mean	Difference: Treated-Control (Reduced Form)	LATE (2SLS)	Control Complier Mean
Employed (=1)	0.463	-0.008 [0.013]	-0.025 [0.041]	0.482
Employed Full-Time (=1)	0.125	-0.003 [0.008]	-0.008 [0.026]	0.125
Earnings	\$4,724.83	\$3.04 [258.574]	\$9.37 [794.805]	\$4,586.11
Earnings (>0)	\$10,214.81	\$256.05 [380.898]	\$788.83 [1173.760]	\$9,263.54
N (Obs.)		33,718	33,718	
N (Individuals)		4,661	4,661	
(c) Re-weighted Demolition Sample: Reduced Form Estimates				
	(1)	(2)		
	Control Mean	Difference: Treated-Control (Reduced Form)		
Employed (=1)	0.430	0.060*** [0.015]		
Employed Full-Time (=1)	0.105	0.013** [0.005]		
Earnings	\$3,891.26	610.087*** [125.687]		
Earnings (>0)	\$9,058.31	\$206.50 [225.969]		
N (Obs.)		35,382		
N (Individuals)		5,246		

Notes: This table displays the results from analyzing the CHAC 1997 lottery and the re-weighted demolition sample. Panel (a) describes household voucher use for households that signed up for the 1997 CHAC housing voucher lottery. Panel (b) analyzes adult labor market outcomes for children (age 7 to 18 at baseline). The control mean statistic in Panel (b) Column (1) refers to averages for children whose household did not win a voucher offer. The reduced form difference is calculated by regressing the outcome (row) on an indicator for winning a voucher offer through the CHAC 1997 lottery. The 2SLS results are obtained by estimating a first-stage where the dependent variable is an indicator for using any housing voucher and the instrument is an indicator for winning a CHAC 1997 housing voucher. Panel (c) revisits the impact of mandatory relocation due to demolition after re-weighting the demolition sample to match observed characteristics in the lottery sample. As explained in the text, I use propensity score weights to achieve balance in observed characteristics between the two samples. Note that the indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

13 Appendix

A1 Detailed Analysis of Differential Attrition

As explained in Section 5.1, I use the following specification for my analysis of the impact of demolition:

$$Y_{it} = \alpha + \beta D_{b(i)} + X_i' \theta + \psi_{p(i)} + \delta_t + \epsilon_{it}$$

where i is an individual and t represents years. The indexes $b(i)$ and $p(i)$ are the building and project where individual i lived. The terms δ_t and $\psi_{p(i)}$ are year and project fixed effects, respectively. The vector X_i is a set control variables that help improve precision by reducing residual variation. The dummy variable $D_{b(i)}$ takes a value of one if an individual was living in a building slated for demolition. Hence, β represents the net impact of demolition on children's outcomes.

One identification condition for this analysis is that $cov(A_{i,t}, \epsilon_{i,t}) = 0$ where A is a binary indicator of attrition. While I do not actually observe A , I follow [Grogger \(2013\)](#) and impute A using various administrative sources. Specifically, the measure of attrition that I calculate is straightforward. Permanent attrition at time t implies that an outcome is zero after the point of departure (i.e. $Y_{i,t+j} = 0 \forall j \in \{1, \dots, T - t\}$, where Y is an administrative data outcome and T denotes the last unit of time in the data). For a single outcome k , I measure attrition by creating a binary indicator of a d -period run of zeros as

$$a_{i,t}^k(d) = \mathbf{1} \left(\sum_{j=0}^{d-1} Y_{i,t+j}^k = 0 \right).$$

Administrative data for the K -many outcomes available across administrative sources can be pooled and attrition can be measured as:

$$a_{i,t}(d) = \mathbf{1} \left(\sum_{j=1}^K a_{i,t}^k(d) = K \right).$$

In what follows, I use the following compact notation: $a_{i,t}^k \equiv a_{i,t}^k(d)$ and $a_{i,t}(d) \equiv a_{i,t}$.

Table A1 shows the distribution of terminal runs of zeros by the year in which the run begins. The first three pairs of columns report statistics based on terminal runs for three different outcomes:

(1) employment, (2) foodstamp receipt and (3) TANF or Medicaid receipt. The first column in each pair reports the probability that a terminal run is observed in a given post demolition year for the sample of non-displaced youth. For example, the first entry of the first column shows that 20.8 percent of non-displaced youth began a terminal run of employment zeros in the first year after demolition. By the definition of terminal run, this sequence was 14 years-long in the first year after demolition. In the second year after demolition, the probability of observing a terminal run of zeros was 21.5 percent. Note that in the second year post demolition, the definition of a terminal run is a 13 year-long sequence. Because the length of the terminal sequence of zeros shrinks in each row, the probability of observing a terminal run of zeros grows over the sample period. Based on the employment data alone, the imputed attrition is 63.1 percent in the final post-demolition year of the sample. Imputed attrition is slightly lower based on data for assistance outcomes as shown in Columns (3) and (5) of Table A1.

Attrition as measured by pooling these administrative sources is reported in Column (7). Combining the three data series dramatically affects the distribution of terminal runs of zeros. Based on the three outcomes, less than 2 percent of the sample begins a terminal run of zeros in the first year after demolition. This contrasts with the 20.8 for employment in isolation. Moreover, attrition based on all three measures is only 30.3 percent in the final year of the sample, which is less than half of the imputed attrition as measured using the employment data alone. This dramatic affect on the distribution is primarily due to the negative correlation among the outcomes under consideration.

The main concern in this analysis is whether demolition appears to be correlated with imputed attrition. For each pair of columns that pertain to a particular outcome in Table A1, the second column of the pair reports the regression computed difference in the probability of attrition for displaced (treated) and non-displaced (control) adolescents who were age 7 to 18 at the time of demolition. Specifically, I use Equation 1 where the outcome is imputed attrition $a_{i,t}^k$. There is no strong evidence of differential attrition by treatment status for any of the single outcomes in isolation. Across the three outcomes in 14 post-demolition years, the difference between the treated and control probability of attrition is statistically significant in just two of the 42 possibilities (5 percent). More importantly, Column (8) shows that there is no detectable difference in the probability of observing a terminal run of zeros in any post demolition year after pooling all three

outcomes.

A2 Detailed Description of Sample Definition

As stated in Section 4, one of the main data sources for this paper is data on social assistance participation from the Illinois Department of Human Services (IDHS). The raw sampling frame for the data used in this paper is the set of individuals (“grantees”) living in Cook County who received some form of social service assistance (specifically, TANF/AFDC, Food Stamps or Medicaid) at *any* point between June 1, 1994 and July 1, 1997. Note that the record for using social assistance during this time period is referred to as the “target case”. With the initial list of grantees, IDHS created a list of other members of the grantee’s household. These additional household members are identified as the set of additional individuals listed on the grantee’s target case. Using this definition for the sampling frame, the raw IDHS data contains 992,729 individuals (463,542 are grantees while 529,187 are individuals living in the same household). Note that everyone in this sample of social assistance households has a unique ID code created by Chapin Hall at the University of Chicago. Chapin Hall uses IDHS data on social assistance utilization to define a unique identifier for individuals who appears in these data. Using information such as name, date of birth and social security number, Chapin Hall used a probabilistic matching technique to link these IDHS-based identifiers to other administrative data such as Illinois state employment data and Illinois State Police (ISP) records, which I also use in the present paper.

A3 Program Rules for Housing Vouchers

A3.1 Voucher Eligibility

Unlike other major social programs, housing vouchers are *not* an entitlement, and there are long waiting lists to receive housing assistance in many large cities. Housing voucher program eligibility is based on the local median household income. For example, a family of four is eligible for assistance if they fall under 50 percent of the local median income for all families in an area (although some families with incomes up to 80 percent of the local median income may be eligible depending on their location) (Olsen, 2003). Note that, unlike other means-tested programs, there are no asset tests for eligibility for housing vouchers. The eligibility limits for families of different sizes are equal to the following percentages of the four-person limit:

Housing Voucher Income Eligibility Adjustment by Family Size (Percentage of Four-Person Limit)

Family Size	1	2	3	4	5	6	7	8
Adjustment	70	80	90	100	108	116	124	132

Notes: All numbers are taken from Olsen (2003), p. 379.

A3.2 The Value of the Subsidy

There are two main components for determining the value of a housing voucher. First, the value of a voucher depends on the local Fair Market Rent (FMR) which is set by the U.S. Department of Housing and Urban Development (HUD). In 1995, the FMR was equal to the 40th percentile of the local rent distribution for a unit of a given size. For example, the FMR for a two-bedroom apartment in Chicago was equal to \$699 (nominal dollars) in 1995. Starting in 2001, the FMR was raised to the 50th percentile in some specific metropolitan areas, including Cook County, Illinois (in which Chicago resides). Second, the value of the voucher depends on household income. Specifically, a fraction of the income – 30 percent – must be paid toward rent. Hence, the value of a housing

voucher is given by:

$$\text{Subsidy Value} = \text{FMR} - S$$

$$S = \max\{0.3 \times Y_{ah}, 0.1 \times Y_{gh}\}$$

$$Y_{ah} = \text{Adjusted income under housing program rules}$$

$$= \text{Earnings} + \text{TANF}$$

$$- (\$480 * \text{Children}) - (\$400 * \text{Disabled})$$

$$- \text{Child care expenses}$$

$$- \text{Medical care expenses}$$

$$- \text{Attendant care expenses for disabled family}$$

$$Y_{gh} = \text{Gross household income}$$

$$= \text{Earnings} + \text{TANF}$$

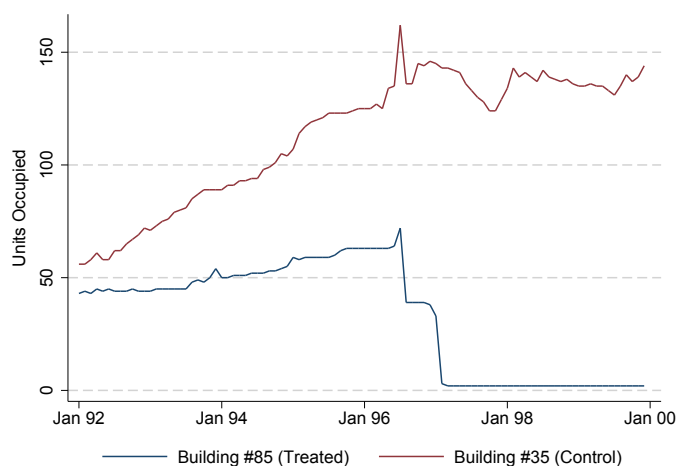
Note that TANF benefits are included when determining program eligibility and the family's rent contribution, while the value of other forms of government assistance such as foodstamps, EITC and Medicaid are not. Earnings by children younger than age 18 or payments received for foster children are also not counted under the voucher program rules. Medical care and attendant care expenses must exceed 3 percent of annual income.

Note that families offered housing vouchers usually have a limited time to lease a private market unit. The time limit is usually 3 to 6 months after initial receipt of the offer. In addition to the time limit, families must also obtain a private-market housing unit that meets HUD's minimum quality standards. As noted in previous work studying vouchers, landlords may prefer non-voucher tenants because of these quality standards or other paperwork associated with the voucher program. Finally, also note that once an individual qualifies for a housing voucher, they are not removed from the program if their income exceeds the eligibility limit. Of course, the value of the subsidy diminishes as income rises because a fraction of household income (generally 30 percent) must be paid toward rent.

A4 Determining Dates of Building Closure Due to Demolition

The date of closure for demolished (treated) buildings used in this paper is taken from [Jacob \(2004\)](#). As explained in the appendix of his paper, Jacob determines the date of building closure by examining trends in administrative data on building-level occupancy rates. Specifically, the year of closure can be determined by *sharp declines* in building occupancy. As an example, the figure below shows how the year of closure is determined from occupancy data. Occupancy at building #85 (the blue line) of the Washington Park project drops notably in early 1996 and later falls to zero starting in 1997. Because CHA policy requires tenants to be notified at least 120 days prior to building closure, the pattern in the occupancy data implies that residents of building #85 knew the building would close in late 1995. As a point of comparison, the figure also shows occupancy at a stable (control) building. We see that occupancy in building #35 (the red line) is relatively stable after 1995 which is the year of the first closures due to demolition at Washington Park. In addition to using administrative data on occupancy, [Jacob \(2004\)](#) also used information from interviews with CHA officials, housing advocates and the presidents of the Local Advisory Councils (LACs) in projects affected by building demolition during the 1990s.

Occupancy Trends and the Date of Building Closure Due to Demolition



Notes: The figure displays monthly occupancy at two buildings at the Washington Park project in Chicago. Occupancy data is from administrative records from the Chicago Housing Authority (CHA).

A5 CHAC 1997 Lottery Data and Summary Statistics

A5.1 Data and Summary Statistics

The data sources used in this paper’s analysis of the CHAC housing voucher lottery have been used previously in [Jacob and Ludwig \(2012\)](#) and [Jacob et al. \(2015\)](#). As mentioned in Section 9, these two studies examine individuals (adults and children) living in private market housing at the time that they applied for the CHAC housing voucher which differs from this paper’s interest in youth living in public housing. The following discussion, which describes the lottery data, is similar to the information provided in the online appendices for [Jacob and Ludwig \(2012\)](#) and [Jacob et al. \(2015\)](#).

The starting point for creating the lottery analysis sample is the application data for the 82,607 adults who applied for a CHAC housing voucher in 1997. These files include information on lottery number and basic household demographics. In addition, the application data also contains baseline address data which allows me to identify the subset of lottery applicants that are the focus of my analysis: applicants living in public housing at baseline. Note that one drawback of the application data is that these data do not contain information on other members of the applicant’s household such as children. Instead, I obtain a list of these household members by linking the application data to the Illinois Department of Human Services (IDHS) data. The procedure for linking applicants to IDHS data is described in greater detail below in A5.2. Note that this linking process uses entirely pre-lottery data, so measurement error in identifying non-applicant household members is orthogonal to winning the housing voucher lottery.

To measure labor market activity, I link data from the Illinois Department of Employment Security (IDES) to the sample of individuals identified in IDHS data as living in lottery households at baseline. The link between the IDES and IDHS data is based on probabilistic matching techniques using name, date of birth and Social Security number. The IDES lets me examine both earnings and labor market participation (defined as having any positive earnings) over time for individuals residing in the state of Illinois.

Similar to my analysis of relocation due to project-demolition, I rely on address information contained in IDHS case records to examine neighborhood outcomes. Recall a concern is that address information is not available when youth are not on active social assistance cases. If this

attrition from the IDHS data is correlated with winning a lottery, it may bias estimates of the impact on neighborhood quality. In the following results section, I show that lottery winning has no statistically significant effect on the probability of having an active social assistance case suggesting that my mobility analysis is not biased by differential sample attrition.

Linking lottery application and administrative data allows me to construct panel data for each youth from the baseline year 1997 to 2009. As mentioned above, I limit my main analysis focuses on the set of lottery applicant households living in project-based public housing at baseline. The sample in my CHAC 1997 analysis has 4,661 children who are between ages 7 and 18 at randomization.

The table on page Appendix - 10 presents summary statistics on my main analysis sample of children. Nearly the entire sample (98 percent) of my sample is African American and lives in a disadvantaged household at baseline. Among adults living in youth households, only 33 percent were employed and average annual income was about \$4,300. Nearly 77 percent of adults received some form of social assistance such as TANF, Medicaid or Foodstamps.

Finally, the key to my analysis is that the CHAC randomly assigned its voucher offers in its 1997 lottery. The descriptive statistics in the table on page Appendix - 10 provide evidence consistent with such random assignment for my main analysis sample of children. The mean values of children and adults in treated and control households are nearly identical. None of the 23 pair-wise differences is significant at the 5 percent level.

Descriptive Statistics for the CHAC 1997 Housing Voucher Lottery Sample

	(1)	(2)	(3)
	Control Mean	Treated Mean	<i>p</i> -value Difference: Treated-Control
Panel A. Children (Age 7-18)			
Demographics			
Black (=1)	0.98	0.98	0.97
Age	11.69	11.81	0.28
Male (=1)	0.49	0.47	0.23
Arrests (Age>13)			
Violent	0.03	0.03	0.49
Property	0.02	0.02	0.41
Drugs	0.05	0.05	0.90
Other	0.03	0.03	0.94
N (Individuals)	3,402	1,300	
Panel B. Adults in Households with Children			
Black (=1)	0.97	0.98	0.78
Age	31.16	31.34	0.47
Male (=1)	0.18	0.2	0.10
Any Arrest	0.77	0.76	0.83
Employed (=1)	0.37	0.38	0.22
Earnings	\$4,340.24	\$4,422.76	0.70
Any social assistance (=1)	0.77	0.75	0.09
N (Individuals)	4,694	1,781	
Panel C. Households with Children			
# of Kids	2.59	2.66	0.31
Neighborhood			
Percent Black	89.14	89.45	0.81
Percent Below Poverty Line	67.14	66.88	0.81
N (Households)	1,464	556	

Notes: All descriptive statistics are for children (age 7 to 18 at baseline) or adults in these households with children.

A5.2 Linking the CHAC Applicants to Other Households Members

Since the CHAC 1997 lottery application form data do not include identifying information for other household members such as children, Jacob and Ludwig contracted Chapin Hall at the University of Chicago to match CHAC applicants to administrative data on social program participation from the Illinois Department of Human Services (IDHS). Chapin Hall matched these two data sources using name, date of birth and Social Security numbers and successfully linked nearly 94 percent of CHAC applicants to the IDHS data. For each CHAC applicant who matched to the IDHS data, Chapin Hall identified the spell of social program participation – referred to hereafter as the “target case” – that was closest in time prior to the date of the CHAC lottery drawing (July 1, 1997). Individuals (such as children) linked to these target cases are counted as residing in the CHAC applicant’s household and included in the lottery analysis sample. For further details on this process of imputing household members see the online appendix for [Jacob et al. \(2015\)](#).

For the present paper, I focus on children (age 7-18 at baseline) who are members of households that reported living in public housing at the time they applied for a housing voucher. Note that baseline residency (address) information is taken from the CHAC 1997 lottery application forms. The list of youth affected by the CHAC lottery is merged to longitudinal administrative data using unique identifiers created by Chapin Hall. These identifiers link individuals across data sources and are created by matching on name, date of birth and social security number.

Specifically, the sample of children living in public housing is merged to the following sources: (1) Illinois State Police (ISP) data recording all arrests up to the first quarter of 2012; (2) Illinois Department of Employment Security (IDES) data on quarterly earnings (1995-2009) and (3) IDHS data on AFDC/TANF, foodstamp and Medicaid participation (1989-2009). Note that these administrative data are also used for the analysis of youth affected by public housing demolition presented in Section 6.

A6 An Economic Model of Reverse Roy Selection in Voucher Programs

This section presents a stylized model of parental investment in child outcomes to explain the pattern of negative selection in the context of a housing mobility program. In particular, the model accounts for two salient features of MTO. First, 80 percent of MTO participants listed fear of crime as their main motivation for joining the program (Orr et al., 2003). In addition, 24 percent of MTO participants reported that someone in their household had been beaten or assaulted in the past six months. This rate of victimization was about four times greater than contemporaneous statistics for other public housing households (Zelon, 1994). Second, fear of neighborhood crime affected parental behavior in MTO households. Kling et al. (2001) interviewed MTO participants before the program and found that “[f]ear has led mothers to constantly monitor their children’s activities.” Notably, studies of MTO showed that the program reduced parents’ active supervising behavior, plausibly because parents who moved felt safer in their new neighborhood (Kling et al., 2001).

A simple model captures this context and generates negative selection into a housing mobility (voucher) program. Assume that each parent living in public housing has a different belief about the safety of their neighborhood q_i . Parents care about their own consumption p_i and their child’s outcome Y_i . Let them believe that their child’s outcome is a function of their parenting effort e_i (e.g., active parental monitoring) and their perception of neighborhood safety q_i . To ensure that parents face tradeoffs, assume that there is a budget constraint: $I = p_i + e_i$.

If parents have no ability to move, they make different investments in their children based on their beliefs about the relative safety (high q_i) or danger (low q_i) of their neighborhood. To make this point clearly, consider the following parameterization of the parent’s preferences:

$$U(p_i, Y_i) = \log(p_i) + \log(Y_i).$$

Parents maximize utility subject to the budget constraint and the child production function which I specify as $Y_i = e_i + q_i$. To optimize, parents choose higher e_i when they have a relatively low q_i . This compensatory behavior is driven by the assumption that parental effort and neighborhood safety are substitutes, which aligns with the behavior of MTO parents who reduced their child monitoring activities after moving to lower poverty neighborhoods.

Now consider how parents respond if an experimental housing voucher program such as MTO program begins recruiting. Assume that the program randomly offers parents the chance to win a housing voucher with probability π . Parents can use the voucher to lease private market housing in a new neighborhood that has high safety θ , where $\theta > \bar{q}$ and \bar{q} denotes the mean of parents' perception of neighborhood safety. In this case, parents with sufficiently low q_i are incentivized to move because they believe their child will live in a much safer neighborhood. Finally, to complete the model, let us assume that there is a utility cost c which ensures that parents with high values of q_i will not want to move through MTO.

In this case, the model fits into the treatment effects framework presented in [Jones \(2015\)](#) and [Pinto \(2015\)](#), and solving the model proceeds in two stages. In the first stage, parents decide whether they want to volunteer ($V_i = 1$) or not ($V_i = 0$). Next, parents choose optimal consumption $p_i^*(D_i)$ and effort $e_i^*(D_i)$ as solutions to a second-stage problem that takes into account whether they are in the program and they receive a voucher ($D_i = 1$). (To be clear, there is no compliance problem in this model. A parent who signs up for MTO and is assigned to the treatment group always uses their voucher.)

Based on the solutions to the second stage, the first-stage decision is a cutoff condition based on the realization of q_i . Specifically, parents predict their consumption in the second stage and choose to volunteer if the following inequality holds:

$$\underbrace{\pi U(p^*(1), y_i^*(1)) + (1 - \pi)U(p_i^*(0), y_i^*(0))}_{\text{Expected Payoff to } V_i = 1} > \underbrace{U(p_i^*(0), y_i^*(0))}_{\text{Payoff to } V_i = 0}$$

Note that the cost c is borne by the parent only if she is assigned to the treatment group and thereby induced to move.

This two-stage model yields a simple expression for the volunteering decision.⁶⁴ Specifically, the log-utility functional form implies that we can re-write the volunteering condition as:

$$\begin{aligned} 2 \log \left(\frac{I - \theta}{I + q_i} \right) &> c \\ \Rightarrow \underbrace{\frac{I + \theta}{\exp^{5c}}}_{\equiv \gamma} &> q_i \end{aligned}$$

In other words, parents with sufficiently poor perceptions of neighborhood safety – below γ – will

⁶⁴Note that the model is solved via backward induction whereby the parent forecasts consumption and utility when they volunteer for MTO.

select into MTO.

With this in mind, we can consider treatment effects generated by the experiment when there are heterogeneous perceptions of neighborhood safety that generate different parental investments. Importantly, note that parents choose to volunteer in the program based on their idiosyncratic signal of neighborhood safety q_i , the mean of which is \bar{q} and is assumed to be the actual level of neighborhood safety. That is, I assume that the actual outcome for a child who does not move is $Y_i = e_i + \bar{q}$. This setup for the model implies that parents with low values of q_i are overly concerned about neighborhood safety.

Now, define the potential outcomes for each child as:

$$Y_{1i} = e_i^*(1) + \theta$$

$$Y_{0i} = e_i^*(0) + \bar{q}$$

where each expression shows that the difference in potential outcomes (and treatment effect heterogeneity) is due to different parental choices for child investment. With selective volunteering, we can use the demand functions to examine treatment effects for children. For the sake of illustration, let us assume that the parent's signal q_i is a normally distributed random variable with mean \bar{q} and variance σ^2 .

In this case, the effects for children of MTO volunteers are:

$$\mathbb{E}(Y_{1i} - Y_{0i} | \gamma > q_i) = .5 \left(\theta - \bar{q} + \sigma \frac{-\phi(\alpha)}{\Phi(\alpha)} \right)$$

where $\alpha = (\gamma - \lambda)/\sigma$. This expression shows that the average treatment effect for participants is decreasing in the standard deviation of parents' perception of safety. Correspondingly, the effects for children of non-volunteers are:

$$\mathbb{E}(Y_{1i} - Y_{0i} | \gamma < q_i) = .5 \left(\theta - \bar{q} + \sigma \frac{\phi(\alpha)}{1 - \Phi(\alpha)} \right)$$

where treatment effects are now increasing in the variance of parents' beliefs about neighborhood safety.

The key point in this model is that the effects for children of non-volunteers exceed effects for children of volunteers. Intuitively, this occurs because parents who select into the experiment would have chosen high levels of e_i if they do not move through MTO. Correspondingly, if these fearful households move via MTO, they reduce e_i because their new neighborhood has relatively high

safety. Again, this effect corresponds to MTO reports, which note that treated parents who moved were less likely to engage in intense parental monitoring relative to parents in control households.

In contrast, parents with high values of q_i are overly optimistic about their neighborhood safety. These households will forgo MTO and choose low values of e_i . This implies that forced relocation would generate large benefits for children because non-volunteers engage in less active child monitoring. Hence, this simple model, which features heterogeneity in beliefs, can explain the Reverse Roy pattern of treatment effects that appears when comparing the impact of vouchers allocated by MTO or Chicago's public housing demolition.

But, is this model reasonable? One way to address this question is to illustrate the quantitative implications of the model by examining treatment effects on child outcomes after calibrating the model. For this illustration, I will focus on the studying effects on adult earnings of children. The model has five parameters: (1) household income I ; (2) private market housing quality θ ; (3) the mean of public housing neighborhood quality \bar{q} ; (4) the variance in signals about neighborhood quality σ^2 ; and (5) the (utility) cost c of moving through a voucher program. To calibrate the model, I assume that the annual household income is \$1,700 which was the mean household income for families living in public housing projects subject to Chicago's demolitions during the 1990s. In addition, I normalize the value of living in private market housing θ to zero.

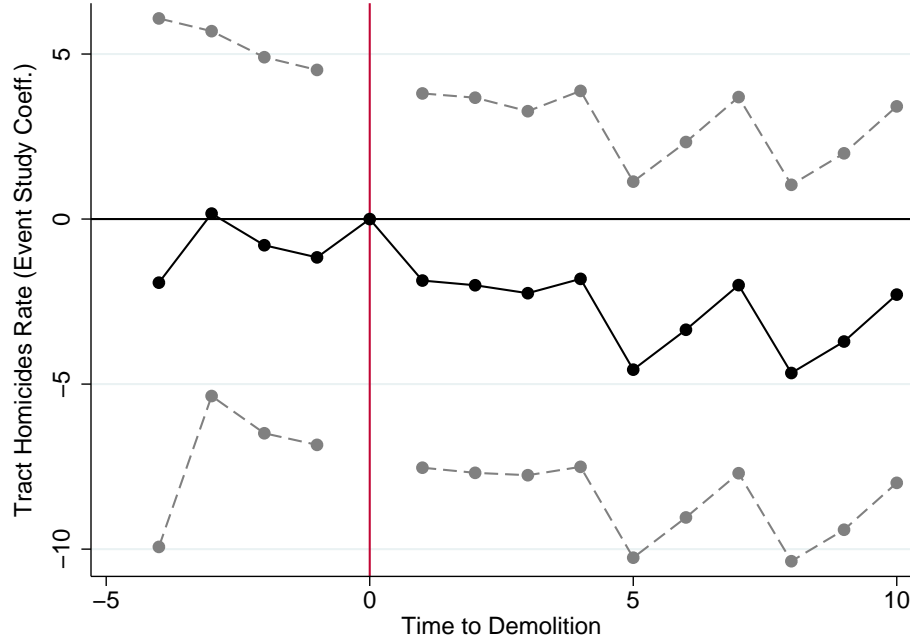
With these assumptions, I can use three moments from the data to solve for the three remaining parameters: \bar{q} , σ and c . First, we have an equation for aggregate volunteering. In the data, we know that about 25 percent of public housing parents opted into the CHAC 1997 lottery. In the model, this implies that the parameters need to be set such that $\Phi((\gamma - \bar{q})/\sigma) = 0.25$. Second, the model should be able to generate the earnings treatment effects observed for children of parents that joined the 1997 CHAC voucher lottery which is about roughly \$10 (although this is an imprecise estimate). In terms of the model, this implies $10 = \mathbb{E}(Y_{1i} - Y_{0i} | \gamma > q_i) = .5(-\bar{q} - \sigma\phi(\alpha)/\Phi(\alpha))$. Similarly, the model should also generate the treatment effects for non-participants. A back of the envelope calculation using the effects observed in the demolition and lottery samples suggests that this is roughly \$770, and this implies: $770 = \mathbb{E}(Y_{1i} - Y_{0i} | \gamma < q_i) = .5(-\bar{q} + \sigma\phi(\alpha)/(1 - \Phi(\alpha)))$.⁶⁵

⁶⁵The effect observed in the demolition sample can be interpreted as the average treatment effect of moving using a voucher which is a weighted average of the effects for children whose parents would *and* would not voluntarily seek vouchers. In terms of the model, we can write this as: $\mathbb{E}(Y_{1i} - Y_{0i}) = \mathbb{E}(Y_{1i} - Y_{0i} | \gamma < q_i)\mathbb{P}(\gamma < q_i) + \mathbb{E}(Y_{1i} - Y_{0i} | \gamma > q_i)\mathbb{P}(\gamma > q_i)$. The estimate for the participation rate in the CHAC 1997 housing voucher lottery and the treatment effects in the demolition and lottery samples imply that the effects for non-participants are \$770.

Solving for these three equations provides the following model parameters: $\bar{q} = 1129$, $\sigma = 906$ and $c = 9.44$. Hence, this simple calibration of this stylized model matches the observed pattern of Reverse Roy selection when there is seemingly moderate variation in parents' beliefs about neighborhood quality (q_i) and a moderate utility cost of moving (c).

A7 Appendix Figures and Tables

Figure A1: Homicide Rate Before and After Demolition: Event Study Coefficients and 95-Percent Confidence Interval

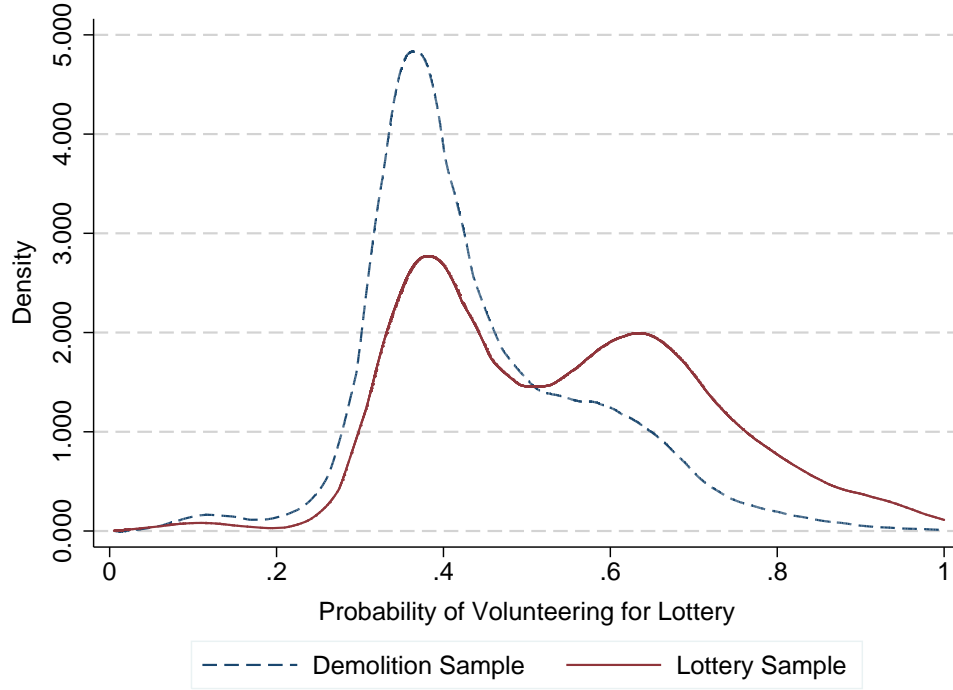


Notes: This figure plots event-study coefficients (solid black dots) from a regression of the tract-level homicide rate on time-dummies. Specifically, the figure plots a set of coefficients π_j and τ_j from the following specification:

$$h_{i,t} = \mu_i + \sum_{j=-4}^{-1} \pi_j \mathbf{1}(t - t^* = j) + \sum_{j=1}^{10} \tau_j \mathbf{1}(t - t^* = j) + \delta_t + \epsilon_{s,t}$$

where the dependent variable $h_{i,t}$ is homicide rate for tract i at year t . The terms μ_i and δ_t are tract and year fixed effects, respectively. In the notation, t^* is the year t in which a particular tract experiences treatment (displacement). The dummy variables $\mathbf{1}(t - t^* = j)$ indicates that an observation in year t is j -periods before or after demolition occurs. For example, the dummy variable $\mathbf{1}(-1 = j)$ indicates that the observation is one year before the policy is implemented. I restrict the estimation sample to include (1) tracts that contained public housing which had at least one building demolition and (2) tracts that are within 1 mile of a public housing demolition site. By definition, all tracts included in this specification are treated at some time. The data contains observations that are at most four years before demolition and up to 10 years after a demolition. I choose four pre-periods because the bulk of the demolitions I consider occur in 1995 and my homicide data start in 1991. Note that year effects δ_t are identified using data from locations that have not yet or already have had a demolition. Grey dots and dashed lines illustrate the 95-percent confidence interval for the coefficients. Data comes from the extended version of Block and Block's Homicides in Chicago (ICPSR #6399).

Figure A2: Propensity Score Distribution



Notes: The figure shows kernel density estimates of the propensity scores for demolition and lottery households, respectively. I construct propensity scores by pooling data on household characteristics for both samples. The unit of observation is at the household level and I estimate a probit with the binary dependent variable equal to 1 if a household selected into the lottery sample. Variables included in the propensity score include baseline measures of the following: (1) the number of criminal arrests (by category for violent, property, drugs and other crimes), (2) household labor market outcomes such as total household income and the fraction of adults that are working in the household, (3) demographic characteristics such as the number of adults and children in the household and (4) measures of past criminal arrests for children. Note that I trim the sample for the figure to exclude propensity scores below 0.01 and above 0.99. These covariates allow me to construct the estimated propensity score $p_i = \mathbb{Pr}(\text{Lottery}_i = 1|X_i)$ which I use to construct weights $w_i = p_i(1 - q)/(1 - p_i)q$ where q is the overall fraction of the pooled household sample that participates in the housing voucher lottery. These weights are used in my analysis of the impact of demolition on long-run child outcomes.

Table A1: Testing for Differential Attrition Using Administrative Data, Child (Age 7 to 18 at Demolition) Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employment		Foodstamps		TANF/Medicaid		All Three Outcomes	
Years Since Demolition (<i>d</i>)	Probability of Attrition by Year <i>d</i>	Difference: Treated– Control, Within Estimate	Probability of Attrition by Year <i>d</i>	Difference: Treated– Control, Within Estimate	Probability of Attrition by Year <i>d</i>	Difference: Treated– Control, Within Estimate	Probability of Attrition by Year <i>d</i>	Difference: Treated– Control, Within Estimate
1	0.208	-0.012 [0.027]	0.068	-0.014 [0.027]	0.054	0.005 [0.020]	0.014	-0.022 [0.017]
2	0.215	-0.012 [0.027]	0.098	-0.015 [0.027]	0.103	0.002 [0.022]	0.027	-0.019 [0.018]
3	0.224	-0.002 [0.027]	0.135	-0.023 [0.026]	0.156	-0.002 [0.024]	0.040	-0.023 [0.019]
4	0.241	0.005 [0.028]	0.165	-0.011 [0.028]	0.200	-0.002 [0.026]	0.054	-0.016 [0.021]
5	0.260	0.012 [0.028]	0.198	-0.003 [0.029]	0.244	-0.002 [0.025]	0.070	-0.011 [0.021]
6	0.283	0.008 [0.028]	0.235	-0.002 [0.028]	0.293	0.028 [0.023]	0.090	0.003 [0.022]
7	0.315	-0.005 [0.028]	0.268	0.003 [0.027]	0.338	0.034 [0.023]	0.111	0.007 [0.023]
8	0.343	-0.003 [0.027]	0.305	0.012 [0.028]	0.394	0.029 [0.025]	0.133	0.016 [0.024]
9	0.377	0.002 [0.028]	0.336	0.022 [0.028]	0.445	0.024 [0.026]	0.157	0.011 [0.025]
10	0.419	0.026 [0.028]	0.380	0.035 [0.029]	0.468	0.029 [0.027]	0.183	0.017 [0.027]
11	0.479	0.054* [0.031]	0.427	0.051* [0.027]	0.489	0.029 [0.028]	0.220	0.039 [0.028]
12	0.471	-0.01 [0.035]	0.441	0.006 [0.024]	0.495	0.003 [0.036]	0.221	-0.011 [0.033]
13	0.550	-0.011 [0.031]	0.490	0.015 [0.029]	0.525	0.004 [0.032]	0.264	-0.018 [0.031]
14	0.631	-0.042 [0.029]	0.525	0.021 [0.031]	0.542	0.016 [0.034]	0.303	-0.015 [0.038]

Notes: This table presents tests for differential attrition based on the administrative data for children (age 7 to 18 at baseline) in my sample. Specifically, I follow [Grogger \(2013\)](#) and construct a measure of attrition based on terminal runs of zeros for a given outcome (e.g. employment) measured in an individual-level panel. For each different outcome, columns (1), (3) and (5) report the probability of observing a terminal run of zeros that begins in a given post demolition year for non-displaced children. For example, the first entry of the first column shows that 20.8 percent of the non-displaced sample of youth began a terminal run of employment zeros in the first year after demolition. Note that for the first entry the definition of a terminal run is a 14 year period. Columns (2), (4) and (6) test whether displaced and non-displaced youth have detectably different rates of attrition. Specifically, these columns report the difference in attrition computed by regressing an indicator for attrition on a dummy for treated (displaced) status and a set of project fixed effects. See Section 4.3 for further details. Columns (7) and (8) examine attrition by pooling data sources.

Table A2: Spillover Specification Results: Adult Outcomes for Children

	(1)	(2)	(3)
	All Children		
	Control Mean	Difference: Treated–Far, Within Estimate (β')	Difference: Near–Far, Within Estimate (π)
Employed (=1)	0.419	0.044** [0.017]	0.005 [0.014]
Earnings	\$3,713.00	\$513.422** [195.356]	\$-122.782 [167.376]
Total Arrests	0.358	-0.031 [0.040]	0.017 [0.027]

Notes: Children are age 7 to 18 at the time of demolition. In the table, “near” refers to the group of children who lived in a public housing building that was adjacent to a building that was demolished while “far” refers to the group of children who lived in public housing buildings that were not adjacent to demolished buildings. The control mean statistics – Columns (1) and (4) – refer to the averages for non-displaced individuals living in the group of far buildings. The regression estimates are from a spillover specification as specified in the text in Equation 2. As described in the text, the estimate β' is the difference in outcomes between displaced and non-displaced children who are part of the far group. Similarly, the estimate π is the difference in outcomes between non-displaced children in the near group and non-displaced children in the far group. Standard errors are presented below each regression estimate and are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$.

Table A3: Earnings Quantile Treatment Effects by Sex

Quantiles							Fraction with Zero Earnings
50	60	70	80	90	95		
Panel A: Descriptive Statistics, Controls							
Male	\$0.00	\$0.00	\$253.57	\$3207.53	\$11,301.13	\$19,269.51	0.67
Females	\$50.07	\$1277.54	\$3841.67	\$8236.44	\$15,409.34	\$21,599.07	0.49
Panel B: Quantile Treatment Effects							
Males	—	—	\$0.00 [13.367]	\$856.296** [408.933]	\$1,314.96 [1,743.996]	\$542.76 [1,312.683]	
Females	\$171.43 [105.731]	\$1,033.82** [104.998]	\$1,877.97** [223.522]	\$2,461.81** [386.654]	\$1,724.63** [607.650]	2,415.52** [787.502]	

Notes: This table presents descriptive statistics and quantile regression results using adult annual earnings data for displaced and non-displaced children (age 7 to 18 at baseline) from public housing projects. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A4: Subgroup Analysis: Impact on Adult Labor-Market Outcomes of Children

(a) Dependent Variable: Labor Participation (=1)			
Subgroup	(1) Fraction of All Children	(2) Employment Control Mean	(3) Employment Difference: Treated–Control, Within Est.
Baseline Age			
<13	0.59	0.374	0.038** [0.017]
13-18	0.41	0.436	0.041** [0.018]
Household Employment			
> 0 Working Adults	0.18	0.454	0.03 [0.032]
No Working Adults	0.82	0.403	0.042** [0.014]
Household Past Arrests			
> 0 Adults with Arrest(s)	0.31	0.39	0.021 [0.028]
No Adults with Arrest(s)	0.69	0.418	0.050** [0.012]
(b) Dependent Variable: Annual Earnings (\$)			
Subgroup	(1) Fraction of All Children	(2) Earnings Control Mean	(3) Earnings Difference: Treated–Control, Within Est.
Baseline Age			
<13	0.59	\$2424.83	\$583.34** [200.505]
13-18	0.41	\$4106.29	\$588.36** [247.348]
Household Employment			
> 0 Working Adults	0.18	\$3,983.29	\$-77.61 [408.349]
No Working Adults	0.82	\$3,305.27	\$723.79** [185.151]
Household Past Arrests			
> 0 Adults with Arrest(s)	0.31	\$2,998.69	\$386.71 [354.330]
No Adults with Arrest(s)	0.69	\$3,571.25	\$713.292** [167.586]

Notes: This table presents results from labor market analysis of subgroups of children defined on baseline (the year before demolition) characteristics. Panels (a) and (b) present subgroup results where the dependent variable in the regression is an indicator for annual employment and earnings, respectively. The control mean statistic in Column (2) refers to the averages for non-displaced individuals. Each specification includes indicators for treatment group interacted with subgroup membership indicators as well as project fixed effects. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

Table A5: Adjusted p -values for Main Demolition Analysis of Adult Outcomes of Children

	(1)	(2)	(3)	(4)
Outcome	Difference: Treat-Control, Within Estimate	Standard Error	p -values	
			Pairwise	FDR- Adjusted
Employed (=1)	0.040	[0.135]	0.0044	0.0040
Earnings	\$602.27	[153.91]	0.0003	0.0003
Any Assistance (=1)	0.128	[0.022]	0.5633	0.5633
Total Arrests	-0.022	[0.024]	0.1628	0.3648

Notes: The results in Columns (3) and (4) are per-comparison (pairwise) and false discovery rate (FDR) adjusted p -values for four main outcomes considered in the analysis of children (age 7 to 18 at baseline) forced to relocate due to building demolition. The FDR-adjusted p -values control for the number of false positives when multiple hypotheses are tested. These adjusted p -values are calculated using the two-step procedure from [Benjamini et al. \(2006\)](#). Columns (1) and (2) repeat the results from Tables 3, 5 and 7 for convenience.

Table A6: Comparing the Short-run Impact of Demolition and Housing Voucher Offers on Neighborhood Characteristics

(a) Demolition Sample				
	(1)	(2)	(3)	(4)
	Control Mean	Difference: Treated-Control (Reduced Form)		
Percent Black	94.897	-0.802 [0.875]		
Percent Below Poverty Line	64.93	-13.871*** [3.124]		
Percent on Public Assistance	58.09	-12.366*** [3.070]		
Percent Unemployed	39.74	-7.848*** [2.047]		
Violent Crime per 10,000 Residents	70.57	-11.300*** [3.118]		
Property Crime per 10,000 Residents	102.87	-0.034 [6.905]		
N (Households)		2,162		
(b) CHAC 1997 Lottery Sample				
	(1)	(2)	(3)	(4)
	Control Mean	Difference: Treated-Control (Reduced Form)	LATE (2SLS)	Control Complier Mean
Percent Black	84.25	2.47 [1.841]	7.45 [5.351]	79.84
Percent Below Poverty Line	45.59	-3.236** [1.550]	-9.771** [4.428]	48.39
Percent on Public Assistance	38.04	-2.719** [1.293]	-8.210** [3.689]	40.45
Percent Unemployed	27.61	-1.790** [0.894]	-5.423** [2.568]	29.52
Violent Crime per 10,000 Residents	32.13	-0.47 [1.075]	-1.44 [3.127]	31.74
Property Crime per 10,000 Residents	69.41	-1.78 [1.853]	-5.43 [5.441]	72.40
N (Households)		1,363	1,363	

Notes: The unit of analysis in this table is a household and there is one observation per household. The dependent variables in each row of the table are neighborhood characteristics measured three years after baseline. This implies that households are only included in the regression if they have valid address (neighborhood) data three years after baseline. Recall that address data is only available if one member of a household actively receives social assistance. Panels (a) and (b) present results for the demolition and CHAC 1997 housing voucher lottery samples, respectively. The control mean statistic – Column (1) – refers to averages for individuals whose household is not displaced by demolition or does not win a voucher offer. The reduced form effect is calculated by regressing each outcome (row) on an indicator for living in a building marked for demolition in Panel (a) or winning a voucher offer in Panel (b). The local average treatment effect (LATE) is estimated using the two-stage system where the dependent variable in the first stage is an indicator for whether a household used a housing voucher and the instrument is an indicator for winning a CHAC 1997 housing voucher.

Table A7: Comparison of Demolition and CHAC 1997 Lottery Households

	(1)	(2)	(3)	(4)	(5)
	Lottery Sample	Demolition Sample	<i>p</i> -value Difference: Lottery– Demolition	Weighted Demolition Sample	<i>p</i> -value Difference: Lottery – Weighted Demolition
(a) Adults in Households with Children (Age 7-18)					
# Adults	1.44	1.15	0.00	1.38	0.01
Single Female Head (=1)	0.69	0.69	0.00	0.70	0.27
Age	31.68	32.16	0.01	31.87	0.28
Earnings	\$ 5,595.30	\$ 1,747.06	0.00	\$ 5,248.41	0.15
Employed (=1)	0.36	0.16	0.00	0.36	0.77
Past Arrests, Any	0.66	0.64	0.01	0.68	0.47
Past Arrests, Violent	0.19	0.16	0.77	0.19	0.95
Past Arrests, Property	0.16	0.15	0.21	0.17	0.72
Past Arrests, Drugs	0.14	0.15	0.02	0.14	0.55
Past Arrests, Other	0.20	0.18	0.37	0.20	0.84
(b) Children (Age 7-18)					
# Kids	2.44	2.10	0.00	2.43	0.86
Past Arrests, Any	0.02	0.04	0.11	0.03	0.26
Past Arrests, Violent	0.01	0.01	0.61	0.01	0.44
Past Arrests, Property	0.01	0.01	0.39	0.01	0.40
Past Arrests, Drugs	0.01	0.02	0.80	0.01	0.67
Past Arrests, Other	0.01	0.01	0.75	0.01	0.62
N (Households)	2,242	2,767		2,767	

Notes: This table compares summary statistics for the demolition and lottery samples and the unit of analysis is at the household-level. Panel (a) presents statistics for adults in households with children (age 7 to 18 at baseline) while Panel (b) presents statistics for children (age 7 to 18 at baseline). Baseline in this context refers to the year before demolition or the year before randomization in the CHAC 1997 housing voucher lottery. Column (4) presents statistics for the demolition sample after re-weighting. The weighting procedure balances sample characteristics between the demolition and lottery samples which is evident from the *p*-values in Column (5). See Section 9.4 for further details on the weighting procedure.

Table A8: Sensitivity of Main Demolition Analysis to Sample Definition

(a) Sample: All Children Ages 5 to 18 at Baseline		
	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.415	0.037*** [0.013]
Employed Full Time (=1)	0.096	0.012** [0.006]
Earnings	\$3,628.97	\$549.582*** [149.769]
Earnings (> 0)	\$8,737.85	\$559.260** [217.636]
N (Obs.)		36,601
N (Individuals)		6,130
(b) Sample: All Children Ages 6 to 18 at Baseline		
	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Employed (=1)	0.417	0.037*** [0.014]
Employed Full Time (=1)	0.097	0.013** [0.006]
Earnings	\$3,659.23	\$565.376*** [152.780]
Earnings (> 0)	\$8,777.10	\$579.157** [219.569]
N (Obs.)		36,223
N (Individuals)		5,752

Notes: This table analyzes adult labor market outcomes for displaced and non-displaced children using different definitions for the sample. Panel (a) uses children age 5 to 18 at baseline while Panel (b) uses children age 6 to 18 at baseline. The control mean statistic – Column (1) – refers to averages for non-displaced individuals. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where a labor market outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression are an indicator for treatment (displaced) status and a set of project fixed effects. See Equation 1 of the text for more details. The indicator variable for “Employed Full Time” is based on whether an individual makes more than \$14,000 in annual earnings – this is the equivalent of 35 hours a week at \$8 per hour for 50 weeks. Robust standard errors are clustered at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$. The analysis omits some observations (less than one percent) that are outliers in the distribution of earnings.

Table A9: Impact on Criminal Arrests of Children

	Panel Model Results	
	(1)	(2)
	Control Mean	Difference: Treated-Control, Within Estimate
Total Arrests	0.362	-0.035 [0.024]
Violent Arrests	0.072	-0.010** [0.004]
Property Arrests	0.034	0.006* [0.003]
Drug Arrests	0.103	-0.005 [0.011]
Other Arrests	0.154	-0.025** [0.011]
N (Obs.)		56,629
N (Individuals)		5,250

Notes: This table analyzes criminal arrests for displaced and non-displaced children. The results here differ from Table 7 because the sample includes all observations where the individual is age 13 or older. Note that the panel for each individual is restricted to the years after demolition. The control mean statistic in Column (1) refers to averages for non-displaced children. The mean difference between displaced and non-displaced children is reported in Column (2). This difference is computed from a regression model where an outcome (each row) is the dependent variable for individual i in year t . The independent variables in the regression are an indicator for treatment (displaced) status and a set of project fixed effects. Robust standard errors are clustered by at the public housing building level. Note that statistical significance is denoted by: * $p < 0.10$, ** $p < 0.05$ and *** $p < 0.01$.

References

- Akerlof, George A. and Kranton, Rachel E.** (2000). ‘Economics and Identity’, *The Quarterly Journal of Economics* 115(3), 715–753.
- Aliprantis, Dionissi and Hartley, Daniel.** (2015). ‘Blowing it up and knocking it down: The local and city-wide effects of demolishing high concentration public housing on crime’, *Journal of Urban Economics* 88, 67–81.
- Anderson, Michael L.** (2008). ‘Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects’, *Journal of the American Statistical Association* 103(484), 1481–1495.
- Angrist, Joshua D.** (2004). ‘Treatment effect heterogeneity in theory and practice*’, *The Economic Journal* 114(494).
- Angrist, Joshua D., Imbens, Guido W. and Rubin, Donald B.** (1996). ‘Identification of Causal Effects Using Instrumental Variables’, *Journal of the American Statistical Association* 91(434), 444–455.
- Benjamini, Yoav, Krieger, Abba M. and Yekutieli, Daniel.** (2006). ‘Adaptive linear step-up procedures that control the false discovery rate’, *Biometrika* 93(3), 491–507.
- Bertrand, Marianne, Luttmer, Erzo F. P. and Mullainathan, Sendhil.** (2000). ‘Network Effects and Welfare Cultures’, *The Quarterly Journal of Economics* 115(3), 1019–1055.
- Bishaw, Alemayehu.** (2014). ‘Changes in Areas With Concentrated Poverty: 2000 to 2010’, *American Community Survey*.
- Black, Sandra E., Devereux, Paul J., Lken, Katrine V. and Salvanes, Kjell G.** (2014). ‘Care or Cash? The Effect of Child Care Subsidies on Student Performance’, *Review of Economics and Statistics* 96(5), 824–837.
- Brooks, Tricia, Touschner, Joe, Artiga, Samantha, Stephens, Jessica, Gates, Alexandra and Stephens, Jessica.** (2015), Modern Era Medicaid, Technical report, Kaiser Family Foundation.
- Census.** (1990), Poverty in the United States, Technical report, Bureau of the Census.
- Chetty, Raj, Friedman, John N., Hilger, Nathaniel, Saez, Emmanuel, Schanzenbach, Diane Whitmore and Yagan, Danny.** (2011). ‘How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star’, *The Quarterly Journal of Economics* 126(4), 1593–1660.
- Chetty, Raj, Friedman, John N. and Rockoff, Jonah E.** (2014). ‘Measuring the Impacts of Teachers II: Teacher Value-Added and Student Outcomes in Adulthood’, *American Economic Review* 104(9), 2633–2679.
- Chetty, Raj, Hendren, Nathaniel and Katz, Lawrence F.** (2015), The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment, Technical report, National Bureau of Economic Research.

- Collinson, Robert, Ellen, Ingrid Gould and Ludwig, Jens.** (2015), Low-Income Housing Policy, Working Paper 21071, National Bureau of Economic Research.
- Coulton, Claudia J., Chow, Julian, Wang, Edward C. and Su, Marilyn.** (1996). ‘Geographic Concentration of Affluence and Poverty in 100 Metropolitan Areas, 1990’, *Urban Affairs Review* 32(2), 186–216.
- Damm, Anna Piil and Dustmann, Christian.** (2014). ‘Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?’, *American Economic Review* 104(6), 1806–1832.
- DHS.** (2014), ‘Illinois DHS – Supplemental Nutrition Assistance Program’.
- DHS.** (2015), ‘Illinois DHS – Work Pays’.
- DiNardo, John and Lee, David S.** (2011), Program Evaluation and Research Designs, in ‘Handbook of Labor Economics’, Vol. 4, Elsevier, pp. 463–536.
- Durlauf, Steven N.** (2004), Chapter 50 Neighborhood effects, in **J. Vernon Henderson and Jacques-Francois Thisse.**, ed., ‘Handbook of Regional and Urban Economics’, Vol. Volume 4 of *Cities and Geography*, Elsevier, pp. 2173–2242.
- Ellen, Ingrid Gould and Turner, Margery Austin.** (1997). ‘Does neighborhood matter? Assessing recent evidence’, *Housing Policy Debate* 8(4), 833–866.
- Finkel, Meryl and Buron, Larry.** (2001), Study on Section 8 Voucher Success Rates: Volume I: Quantitative Study of Success Rates in Metropolitan Areas, Technical report, Abt Associates.
- Fredriksson, Peter, ckert, Björn and Oosterbeek, Hessel.** (2013). ‘Long-Term Effects of Class Size’, *The Quarterly Journal of Economics* 128(1), 249–285.
- Fryer, Roland G., Jr. and Katz, Lawrence F.** (2013). ‘Achieving Escape Velocity: Neighborhood and School Interventions to Reduce Persistent Inequality’, *The American Economic Review* 103(3), 232–237.
- Galster, George, Marcotte, Dave E., Mandell, Marv, Wolman, Hal and Augustine, Nancy.** (2007). ‘The Influence of Neighborhood Poverty During Childhood on Fertility, Education, and Earnings Outcomes’, *Housing Studies* 22(5), 723–751.
- Garza, Melita.** (1999a). ‘9 High-rises At Taylor Homes Slated To Close’, *Chicago Tribune* .
- Garza, Melita.** (1999b). ‘CHA Evacuates High-rise Units Without Heat’, *Chicago Tribune* .
- Goering, John, Kraft, Joan, Judith, Feins, McInnis, Dennis, Holin, Mary and Elhasan, Huda.** (1999), Moving to Opportunity for Fair Housing Demonstration Program: Current Status and Initial Findings, Technical report, U.S. Department of Housing and Urban Development.
- Grogger, Jeffrey.** (2013), Bounding the Effects of Social Experiments: Accounting for Attrition in Administrative Data, Working Paper 18838, National Bureau of Economic Research.
- Hastings, Justine, Kane, Thomas and Staiger, Douglas.** (2008). ‘Heterogeneous preferences and the efficacy of public school choice’, *NBER Working Paper* 2145.
- Hawes, Christine.** (1992). ‘Now Things Move Quickly At Cabrini’, *Chicago Tribune* .

- Heckman, James, Tobias, Justin L. and Vytalil, Edward.** (2001). 'Four Parameters of Interest in the Evaluation of Social Programs', *Southern Economic Journal* 68(2), 211–223.
- Jacob, B. A., Kapustin, M. and Ludwig, J.** (2015). 'The Impact of Housing Assistance on Child Outcomes: Evidence from a Randomized Housing Lottery', *The Quarterly Journal of Economics* 130(1), 465–506.
- Jacob, Brian A.** (2004). 'Public Housing, Housing Vouchers, and Student Achievement: Evidence from Public Housing Demolitions in Chicago', *The American Economic Review* 94(1), 233–258.
- Jacob, Brian A and Ludwig, Jens.** (2012). 'The Effects of Housing Assistance on Labor Supply: Evidence from a Voucher Lottery', *American Economic Review* 102(1), 272–304.
- Jacobson, Louis S., LaLonde, Robert J. and Sullivan, Daniel G.** (1993). 'Earnings Losses of Displaced Workers', *The American Economic Review* 83(4), 685–709.
- Jones, Damon.** (2015), The Economics of Exclusion Restrictions in IV Models, Working Paper 21391, National Bureau of Economic Research.
URL: <http://www.nber.org/papers/w21391>
- Kline, Patrick and Walters, Christopher.** (2014), Evaluating public programs with close substitutes: The case of Head start.
- Kling, Jeffrey R., Liebman, Jeffrey B. and Katz, Lawrence F.** (2001), Bullets Dont Got No Name: Consequences of Fear in the Ghetto, Working Paper 274, Princeton University, Woodrow Wilson School of Public and International Affairs, Center for Health and Wellbeing.
- Kling, Jeffrey R., Ludwig, Jens and Katz, Lawrence F.** (2005). 'Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment', *The Quarterly Journal of Economics* 120(1), 87–130.
- Ludwig, Jens, Duncan, Greg J. and Hirschfield, Paul.** (2001). 'Urban Poverty and Juvenile Crime: Evidence from a Randomized Housing-Mobility Experiment', *The Quarterly Journal of Economics* 116(2), 655–679.
- Massey, Douglas S. and Denton, Nancy A.** (1993), *American Apartheid: Segregation and the Making of the Underclass*, Harvard University Press.
- Newman, Oscar.** (1973), *Defensible Space: Crime Prevention Through Urban Design*, Collier Books.
- Olsen, Edgar O.** (2003). 'Housing Programs for Low-Income Households', *NBER* pp. 365–442.
- Olsen, Edgar O.** (2014), Alleviating Poverty through Housing Policy Reform.
- Oreopoulos, Philip.** (2003). 'The Long-Run Consequences of Living in a Poor Neighborhood', *The Quarterly Journal of Economics* 118(4), 1533–1575.
- Orr, Larry L., Feins, Judith, Jacob, Robin, Beecroft, Erik, Sanbonmatsu, Lisa, Katz, Lawrence F., Liebman, Jeffrey B. and Kling, Jeffrey R.** (2003), Moving to Opportunity Interim Impacts Evaluation, Technical report, U.S. Department of Housing and Urban Development.

- Pinto, Rodrigo.** (2015), Selection Bias in a Controlled Experiment: The Case of Moving to Opportunity.
- Popkin, Susan J.** (2012), Public Housing Transformation and Crime: Making the Case for Responsible Relocation, Research Publication, Urban Institute.
- Popkin, Susan J, Gwiasda, Victoria, Olson, Lynn, Rosenbaum, Dennis and Buron, Larry.** (2000), *The Hidden War: Crime and the Tragedy of Public Housing in Chicago*, Rutgers University Press, New Brunswick, NJ.
- Popkin, Susan J., Rosenbaum, James E. and Meaden, Patricia M.** (1993). ‘Labor market experiences of low-income black women in middle-class suburbs: Evidence from a survey of Gautreaux program participants’, *Journal of Policy Analysis and Management* 12(3), 556–573.
- Rosenbaum, James E.** (1995). ‘Changing the geography of opportunity by expanding residential choice: Lessons from the Gautreaux program’, *Housing Policy Debate* 6(1), 231–269.
- Sampson, Robert J. and Groves, W. Byron.** (1989). ‘Community Structure and Crime: Testing Social-Disorganization Theory’, *American Journal of Sociology* 94(4), 774–802.
- Sanbonmatsu, Lisa, Kling, Jeffrey R., Duncan, Greg J. and Brooks-Gunn, Jeanne.** (2006). ‘Neighborhoods and academic achievement results from the Moving to Opportunity experiment’, *Journal of Human Resources* 41(4), 649–691.
- Sanbonmatsu, Lisa, Ludwig, Jens, Katz, Lawrence F., Gennetian, Lisa A., Duncan, Greg J., Kessler, Ronald C., Adam, Emma, McDade, Thomas and Lindau, Stacy.** (2011), Moving to Opportunity for Fair Housing Demonstration Program: Final Impacts Evaluation, Technical report, U.S. Department of Housing and Urban Development.
- Shroder, Mark D. and Orr, Larry L.** (2012). ‘Moving to Opportunity: Why, How, and What Next?’, *Cityscape* pp. 31–56.
- Vale, Lawrence J. and Graves, Erin.** (2010). ‘The Chicago Housing Authority’s Plan for Transformation: What Does the Research Show So Far’, *Massachusetts Institute of Technology, Department of Urban Studies and Planning*.
- Walters, Christopher R.** (2014), The demand for effective charter schools, Working Paper 20640, National Bureau of Economic Research.
- Wilson, William J.** (1987), *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*, University of Chicago Press.
- Zelon, Harvey.** (1994), Survey of Public Housing Residents: Crime and Crime Prevention in Public Housing, Technical report, Research Triangle Institute, Research Triangle Park, NC.